55227/B

An extremely important work in the history of science. Yeats, like his contemporary Beddoes, drew attention to the merits of Mayow and though, says E. Irving Carlyle, "like most of Mayow's admirers Yeats applauded with too little discrimination, he assisted in the rescuing of his achievements from oblivion."

Yeats was an American born in Florida who took his medical degrees in England. This is his most important work.

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JOHN MAYOW.

OBSERVATIONS

ON THE

CLAIMS OF THE MODERNS,

TO SOME

DISCOVERIES IN CHEMISTRY

AND

PHYSIOLOGY.

BY G. D. YEATS, M. B.

OF HERTFORD COLLEGE, OXFORD, MEMBER OF THE
ROYAL MEDICAL SOCIETY OF EDINBURGH,
AND PHYSICIAN AT BEDFORD.

> *De verbis autem cavilliſque cum nemine unquam libuit altercari, quærenti deriſori ſcientia ipſa ſeſe abſcondit, ſed ſtudioſo veritatis obviam venit et ſe præbet conſpiciendam.* WALÆUS.
>
> *Tros Tyriuſve mihi nullo diſcrimine agetur.* VIRGIL.

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TO

DR. MAYO, DR. VAUGHN,

AND

DR. P. MAYO,

PHYSICIANS TO THE

MIDDLESEX HOSPITAL,

THE FOLLOWING WORK IS INSCRIBED,

AS A MARK OF ESTEEM,

BY THEIR SINCERE FRIEND,

THE AUTHOR.

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INTRODUCTION

TO

PART THE FIRST.

When I became acquainted with the more recent theories in chemiſtry and phyſiology, I was taught to believe, that they had been excluſively broached by the philoſophers of the preſent day; but having applied myſelf to the reading of authors of more early date, I was ſurpriſed to find, that not only many of the modern theories had been delivered and explained, but alſo, that a conſequent practice had been adopted and followed. The more I examined into their writings, the more was I confirmed in the belief, that the labours of many ingenious men had been unaccountably overlooked, or greatly miſrepreſented. Chemiſtry, in particular, which has blazed with ſuch ſuperior luſtre from the Continent, forms a diſtinguiſhed portion of the effects of modern inveſtigation.

The diſcovery and decompoſition of the different gaſes, will give a permanency to that branch of ſcience; and which, with ſome theories in phyſiology, very much contribute to revive and confirm the opinions of many eminent men, of the laſt and preſent century. This aſſertion will, no doubt, cauſe ſurpriſe in ſome; but if they will candidly and impartially examine the ſubject, they muſt be convinced of the fact. It is certainly, *primâ facie*, a matter of aſtoniſhment, how it happens, that theories and experiments, clearly and fairly explained, ſhould have eſcaped the obſervation of medical men, till more recent inveſtigation attracted their attention to the ſubject. It is difficult to account for the prejudice of the human mind, with its conſequent rooted attachment to eſtabliſhed ſyſtems; but there, certainly, is evident, in the popular mind, a proneneſs, at particular periods, to the reception of certain ſyſtems, in like manner as there exiſts in the body, a prediſpoſition to diſeaſe, at different times. That cuſtom powerfully operates to confirm prejudices, however abſurd, and to enſlave the mind to theories, however ridiculous, is evident from many diſtinguiſhed æras in the annals of philoſophy. When men have ſpent, probably, a long life in perſuading themſelves that a particular ſyſtem is true, they receive, with reluctance, any alterations from the more ſucceſsful labours of cultivated genius. The reliance upon a certain remedy, becauſe it has ſometimes ſucceeded; the attachment to a particular theory, becauſe they have been taught to believe it; the envy, perhaps, of not being themſelves the diſcoverers; all conſpire to raiſe the ſpirit of oppoſition, againſt the intruſion of philoſophical innovation. ‘ Multi ob fauſtos eventus ſemel aut bis obſervatos ex aliquo remedio, vel potius ob innatam quendam proclivitatem tum laudendi tum fingendi, ad libitum medicamentorum vires ita erga remedium aliquod efficiunter, ut putent, illud unum ad curandos quoſque morbos ſummam vim ac veluti imperium obtinere *.

I need ſcarcely obſerve, how lamentably often this has happened, and how long it obſtructed the progreſs of medical knowledge. Notwithſtanding the diſcovery of the valves of the veins, by Fabricius,

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* Baglivi de Praxi Medica. P. 18.

Fabricius, Ceſalpinus, and others, ſo wedded were medical men to the Galenic doctrine, that they not only had no idea of their uſe, but laboured to explain them, as ſubſervient to that theory; and the liver ſtill maintained its pre-eminence in the ſyſtem. Circumſtances, like theſe, ſeem long to have retarded the knowledge of the circulation, and after our eminent countrymen demonſtrated, with the preciſion of mathematical truth, his brilliant diſcovery, the partial admiration of Galen produced ſo violent an oppoſition, that it prevented the diffuſion of the diſcovery, with that celerity, which its great importance required.

Theſe thoughts naturally preſſed themſelves upon my mind, when I reflected, with what coolneſs the beautiful experiments of Mayow were received, upon their publication. Retired from the world, he planned and executed, in the cloiſters of a college, experiments the moſt elegant and deciſive, that the greateſt genius could contrive. Unaſſiſted by the labours of others, not encouraged by the adoption of his opinions, his aſpiring genius ſoared into the regions of truth, amid the obſtacles of ſurrounding oppoſition. Every one who is acquainted with Mayow's writings, and the ſpirit of his expreſſions, muſt acknowledge and regret, that his early death was a great loſs to ſcience*. Had he lived, he would, no doubt, have preſſed his accumulated truths upon the minds of men, with reſiſtleſs evidence.

It is an obſervation, juſtified by the pages of philoſophy, that the adoption of ſyſtems, is the reſult of the preparation of the minds of men, by a continued attention to the phænomena of nature. The labours of an individual, however ſucceſsful, will not ſuffice to convince the erroneous, or to reclaim the bigotted. It appears, that a particular ſtimulus is wanting, to direct, to the particular object, that general attention, which the brilliancy of genius cannot always command. A revolution in ſcience, is always attended with a revolution in terms; and theſe are often applied, from a particular theory: hence the difficulty, without a painful and laborious attention, of arriving at the ſpirit in the expreſſion of the diſcoverers. But when experiments are made, by ſeveral philoſophers, quickly ſucceeding each other, the mind is prepared, by a *partial* acquaintance with terms; and, therefore, receives, with leſs reluctance, the full completion of the diſcoveries. The quick ſucceſſion of ingenious experiments, from the induſtry of Hales, of Prieſtley, and of Black, paved the way, and cauſed a ready reception, for the beautiful ſyſtem of Lavoiſier; and had not anatomy directed the attention of medical men, to the diſcovery of the valves of the veins, immediately previous to Harvey, the knowledge of the circulation would, probably have remained, for ſome future examiner into the operations of the animal œconomy. When it remains for an individual, whoſe acute obſervations is not ſatisfied with the contradictions in an eſtabliſhed ſyſtem, to plan and execute a revolution in ſcience, he may expect a cold reception, and for a time, a rejection of his opinions. When, however, from the evolution of events, and a more minute acquaintance with natural phænomena, the mind is, in ſome degree, diveſted of its prejudices, the new ſyſtem begins to undergo inveſtigation, and if conſiſtent with truth, acquires its votaries from candour and impartiality.

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* Vide Wood's Athenæ Oxon.

partiality. This ſeems to have been preciſely the caſe with Mayow. His ſyſtem ſuddenly burſt upon the literary world, with all that beauty and energy, which adorn the pages of the modern chemiſt. His terms perplexed, becauſe they were not properly ſtudied, and his doctrines were neglected, becauſe, at that time, ſevere attention was neceſſary to underſtand them.

When a ſyſtem of ſcience, which future diſcoveries prove to be falſe, has been adopted and believed, for ages, we are naturally ſurpriſed to find, that errors, ſo groſs, ſhould have been maintained, or that facts, ſo ſimple and obvious, ſhould have eſcaped obſervation. The unprejudiced mind, engaged in its purſuits, beholds, with a mixture of ſurpriſe and curioſity, the diſcoveries of the ingenious, and contributes its exertions to examine theſe new opinions. Novelty, indeed, with ſome, independent of the importance of facts, has a conſiderable effect in enchanting the mind; and when aided by the charms of eloquence, and enforced by the powers of argument, it enſlaves us often to prejudices and contradictions. But when time has wiped away the force of novelty, and the efferveſcence, cauſed by the promulgation of the diſcoveries, has ſubſided, the mind retires, to examine into their authenticity, and to award its juſtice. When we find, from enquiry, that the new diſcoveries have ariſen from objects, daily preſented to our obſervation, we are naturally led to examine, how far the ingenious, of paſt ages, were acquainted with them; and if they poſſeſſed the knowledge, what obſtacles prevented its completion, or retarded its tranſmiſſion to poſterity. Speculations, like theſe, prompt us to trace, in authors, the opinions of paſt ages; and free from the bias of party zeal, we can make a candid enquiry, and give an impartial view of the merits of the diſcoverers. The ſimplicity which adorns the late diſcoveries in chemiſtry, will naturally ſuggeſt ſuch reflections, and will induce us to imagine, that phænomena, ſo ſtriking and obvious, muſt have arreſted the attention of many ingenious men, who flouriſhed in paſt ages; accordingly, we find in many, the moſt pointed remarks, and the cleareſt expreſſions, accompanied by proofs, by which we cannot but believe, they underſtood a conſiderable part of modern chemiſtry.

The doctrine of the convertibility of air into other ſubſtances, and *vice verſâ*, is very antient. Even as far back as the time of Pythagoras, we trace a remarkable co-incidence between his doctrine, and that of modern times. It is needleſs for me to mention, that we have no writings, either of Pythagoras himſelf, or of any of his immediate ſcholars: of courſe, our notion of his philoſophy, muſt be taken from the occaſional mention of him and his opinions, in the antient philoſophers and poets. The doctrine of this ſage, cannot be better explained to the reader, than by quoting the paſſage, concerning it, from the fifteenth book of Ovid's Metamorph.

Hæc quoque *non perſtant*, quæ nos *elementa* vocamus.
Quaſque vices peragant, animos adhibete, docebo.
Quatuor æternus genitalia corpora mundus
Continet. Ex illis duo ſunt oneroſa, ſuoque
Pondere in inferius, tellus atque unda, feruntur;
Et totidem gravitate carent: nulloque premente
Alta petunt, aer atque aere purior ignis.
Quæ quanquam ſpatio diſtant, tamen omnia fiunt
Ex ipſis, et in ipſa cadunt: reſolutaque tellus
In liquidas roreſcit aquas, tenuatus in auras
Aeraque humor abit; dempto quoque pondere rurſus

In ſuperos aer tenuiſſimus emicat ignes.
Inde retro redeunt, idemque retexitur ordo.
Ignis enim denſum ſpiſſatus in aera tranſit,
Hic in aquas, tellus glomeratâ cogitur unda.
Nec ſpecies ſua cuique manet: rerumque novator,
Ex aliis alias reparat natura figuras.
Nec perit in tanto quidquam (mihi credite) mundo,
Sed variat, faciemque novat, naſcique vocatur,
Incipere eſſe aliud, quam quod fuit ante: morique
Deſinere illud idem. Cùm ſint huc forſitan illa
Hæc tranſlata illuc; ſumma tamen omnia conſtant.

V. 237.

The ingenious Dr. Gregory, who fills, with ſuch diſtinguiſhed merit, the practical chair at Edinburgh, in anſwer to a letter which we wrote to him, makes the following obſervations. ‘ The co-incidence between the doctrine in this paſſage, and ſome of the moſt modern chemical diſcoveries, eſpecially thoſe about air and water, appears to me very ſtriking. I reckon it merely accidental. But alluding to the admiration of the antients, which prevailed for two centuries and more, after the revival of letters, and the eagerneſs which ſome learned men ſhewed, to refer to the antients every modern diſcovery and improvement, I ſaid, in my jectures, 'There was a time when, upon the faith of that coincidence, the credit of our moſt important modern diſcoveries in chemiſtry, would have been transferred from the real author of them to Pythagoras, or perhaps to Hermes.'

The convertibility of different ſubſtances into each other, was attended to, we ſee, by the Pythagorean ſchool. Lucretius, too, has mentioned this doctrine, in his work De Rerum Natura; and ſays, that air is conſtantly changing into other ſubſtances, which are again decompoſed into air; and were it not for this continual round, every thing, at laſt, would remain converted into air.

Aera nunc igitur dicam, qui corpore toto
Innumerabiliter privas mutatur in horas,
Semper enim quodcumque fluit de rebus, id omne
Aeris in magnum fertur mare, qui niſi contra
Corpora retribuat rebus recreetque fluenteis,
Omnia jam reſoluta forent et in aera verſa,
Haud igitur ceſſat gigni de rebus, et in res
Recidere aſſidue, quoniam fluere omnia conſtat.*

This

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* Titi Lucretii Cari, de Rerum Nat. Lib. V. v. 274. Francofurti, 1583.

This idea, no doubt, firſt preſented itſelf to this ſect, from obſerving water to eſcape in a ſtate of vapour, and from attending to its ſubſequent condenſation. The different fermentations ſhewed to them, that ſolid bodies were reducible into fluid maſſes, and that, during this converſion, a conſiderable quantity of *vapour* eſcaped; hence they concluded, and not without reaſon, that theſe ſubſtances were changed into air. Water, they ſaw, in a ſolid, fluid, and vaporific ſtate, according to the degree of heat; but they were not aware of the cauſe of theſe different ſtates. It was imagined, then, by the Pythagoreans, to ſpeak in modern language, that all ſubſtances were compoſed of the ſame elementary principles, in different ſtates of combination. By ſpontaneous changes, theſe were reſolvable, reciprocally, into each other, according as circumſtances varied. A ſpontaneous decompoſition was continually going on, which varied the face of the creation; nature was employed, in one continued circle of operations; and the various objects which preſented themſelves to their obſervations, were changed, modified, and renewed; and thus originated the doctrine of the Samian ſage. Theſe, were facts acquired from the experience of a long courſe of years, and, by no means, the reſult of a particular enquiry. A philiſophy like this, too, would have been the firſt to be adopted, by an attentive obſerver, as it aroſe from facts, the moſt obvious and the moſt ſtriking; accordingly, we find the principles of it traced to the earlieſt period.*

With notions like theſe, it is not difficult to underſtand the origin of metempſychoſis. The Pythagoreans contended, that nothing was periſhable: ſubſtances, which appeared to be deſtroyed, by no means ſuffered a total annihilation; it was only a modification of form. A tranſition, from the material to the immaterial part of the creation, was eaſy; the analogy was ſtriking, the concluſion inevitable. This doctrine was regular and ſyſtematic, in every branch of it; it compriſed a complete and uniform plan of philoſophical principles; and as it wore the appearance of truth and ſimplicity, and was delivered out with the authority of

age

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* It is ſaid, that Pythagoras borrowed the principles of his doctrines from the Gymnoſophiſts, or Indian Brahmins.

age and experience, it required numerous votaries, and enſlaved the mind, for many ſubſequent centuries.

That admirable philoſopher, Mr. Boyle, imagined, that water might be converted into air. He relates an experiment, which he ſays he made, in conſequence of effects produced by a machine, which Kircher had invented, and by which he ſuppoſed the water was changed into air. He took a glaſs veſſel, capable of containing about three ounces: He filled it with oil of vitriol, diluted with half its quantity of water, and having thrown in ſome pieces of iron, he inverted it into another veſſel, containing the ſame fluid. Air was ſoon diſengaged, in conſiderable quantity; and the acid, of courſe, would be proportionably depreſſed. This air, he ſays, very ſoon filled the inverted glaſs; and it is worthy of remark, he adds, that when he applied his warm hand to the glaſs, containing the air, it expanded very rapidly. This experiment, he ſays, although it does not abſolutely prove, that air is produced from water, yet it ſhews, beyond a doubt, that air may be generated *de novo*. The reader knows, that it was hydrogen which Boyle here procured, one of the conſtituent principles of water. What induced him to add water to the acid, in this experiment? Was he aware, that the oil of vitriol did not act upon iron ſo rapidly, as when united with water? Certain it is, that the water was added, from Boyle's own words, with a view to decompoſition. How beautifully has this truth been confirmed, by the experiments of the Honorable Mr. Cavendiſh, who firſt diſunited the component parts of water. Little has it been immagined, however, that ſimilar ideas were entertained, previous to the preſent age. The ingenious Mr. Boyle made many experiments on animals, with his air-pump. It will be proper to ſay ſomething of them, as they involve many important facts, and as Mayow quotes them in his work.

It is well known that the Honorable Mr. Boyle was conſidered, as one of the firſt experimentaliſts of the age in which he lived. He invented a variety of ingenious contrivances, for carrying on his experiments. The moſt ingenious, as well as the moſt uſeful, was the air-pump, which he conſiderably improved from Otto de Guerick, its celebrated inventor, and which firſt inconteſtibly proved, that animals could not live, one moment of time, without the preſence of air. He found, that a variety of inſects, as flies, bees, worms, &c. were immediately deprived of the power of motion, by exhauſting the air-pump, and that mice, ſparrows, &c. died, when placed in the ſame ſituation. He made the experiments with fiſh, with the ſame reſult; and obſerved, that they turned upon their backs*. Such remarkable phænomena could not paſs without a comment, from ſo acute a philoſopher as Boyle; he ſubjoins ſome obſervations (doubts as he calls them) on reſpiration, ſo intimately connected with theſe experiments. He modeſtly confeſſes, that his experiments are not ſufficiently numerous or ſatisfactory, to warrant poſitive aſſertions; but they have ſuggeſted to me, he ſays, ſome new ideas, with reſpect to the uſe of air, by which my former diſtruſt of the general received opinion, is much ſtrengthened; I am, therefore, inclined to ſeize the preſent opportunity, and relate my doubts.

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* This fact is very ingeniouſly explained by Mayow, as the reader will ſee, by turning to Sect. XIV. P. 255.

That reſpiration is not intended to cool and condenſe the blood, as many philoſophers of the firſt reſpectability imagine, is evident, from many facts; but particularly from obſerving, that to animals of cold blood, and old people, whoſe circulation is languid, and heat deficient, reſpiration is neceſſary; alſo, in ſome diſeaſes, animal heat is ſo much exhauſted, that were reſpiration to cool the blood, much injury would be done. There are others, of the firſt note, who imagine, that the very ſubſtance of the air enters the lungs, and paſſes to the left ventricle, not only to cool the blood, but alſo to prepare the way for the generation of animal ſpirits. This opinion he does not admit, although it comes neareſt to the truth of any. It is the opinion of others, again, that reſpiration is intended to paſs the blood, from one ventricle to the other, by which it is enabled to diſcharge much noxious effluvia*, which keeps up the vigour of the circulation, in a ſimilar way, as

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[*] Even after the revolution of more than a century, this opinion continued to prevail; and Dr. Prieſtley endeavoured to confirm it, after his diſcoveries. Vide Vol. III. P. 55; and Kirwan on Phlogiſton.

as a free egreſs to the ſmoke cauſes a fire to burn well. He agrees, in part, with this opinion; and relates an experiment, in confirmation of it. He confined a bird, under a glaſs veſſel, and it died in about three quarters of an hour; this happens, he ſays, becauſe the exhalations from the body have ſo infected the air, as to render it unfit for reſpiration. This is not to be wondered at, he obſerves, becauſe the inſenſible perſpiration exceeds the other diſcharges of the body. Notwithſtanding this, it may be reaſonably ſuſpected, that the air ſerves other purpoſes, beſides carrying off effluvia from the lungs; we may therefore conclude that there is ſome other uſe of the air, not yet diſcovered, which makes it ſo uninterruptedly neceſſary to the life of animals. Paracelſus, indeed, obſerves, that as the ſtomach digeſts the food, and converts one part to the uſe of the body, and rejects the other; ſo the lungs conſume one portion of the air, but does not admit the other. It would appear, therefore, that we ſhould agree with this philoſopher, that there is a certain vital elixir in the air, neceſſary for reſtoring our vital ſpirits.

I take the preſent opportunity, ſays Boyle, of relating the invention of Cornelius Debrell, a man deſervedly celebrated, on account of his ſkill in chemiſtry and mechanics. It is related by many of the firſt credit, that he built, for our late King James, a ſmall boat, ſo conſtructed, as to be rowed under water, which experiment was made, with wonderful ſucceſs, in the river Thames. This boat carried twelve rowers, beſides paſſengers, one of whom is living at this day, and related the ſtory to a great mathematician, from whom I got it. I alſo mention it, ſince neither curioſity has been wanting on my part, nor have I let ſlip any opportunity, of making a particular enquiry among the relations of Debrell; but chiefly I enquired of an ingenious phyſician, who married his daughter, how it could poſſibly be effected, that men ſhould remain ſo long under water, with perfect impunity. I was told, that Debrell imagined, that it was not the whole body of air, but a certain quinteſſence, or ſpirituous part of it, which ſerved reſpiration, and which being conſumed, the remaining part of the air became effete, or lifeleſs, ſo that it was unable to maintain the vital flame, reſiding in the heart. Therefore (as far as I could procure intelligence) beſides the mechanical contrivances of a veſſel, he had a certain chemical liquor, which was the great ſecret in this ſubmarine navigation; for as often as he perceived the purer part of the air was conſumed, or too much injured by reſpiration, and infected with the effluvia of the navigators, he opened the veſſel containing this fluid, which ſuddenly reſtored to the injured air ſo much of its vital parts, as enabled it to ſerve the purpoſes of reſpiration, for ſome time longer. Boyle ſays, that Debrell never told any, of what this curious liquor was compoſed. 'I have related this ſtory,' ſays this ingenious philoſopher, 'not becauſe I think it altogether true, but becauſe it is not mentioned by any author, nor is it generally known. With reſpect to reſpiration, I am ſometimes inclined to agree with thoſe, who imagine, that the air is neceſſary to maintain the vital flame in the heart; for in our experiments with the air-pump, when the air is exhauſted, flame will not continue much longer than the life of an animal.' Although our experiments ſhew a *new* ſimilarity between life and combuſtion, yet this opinion is oppoſed by many

difficulties;

difficulties; for although in the hearts of animals the blood is warm, it is impoſſible to conceive, how the air can arrive at the heart, and if it did, by what means it can increaſe the heat; for although the air increaſes the heat of burning coals, yet being blown on warm liquors, it cools them.

Such are the doubts which Boyle has propoſed, without determining for any one opinion. How near was he to the truth, when he obſerved, that there was a ſimilarity between life and combuſtion. Here experiments were wanting; and had Boyle varied his, he might have diſcovered the cauſe of animal heat. He did nothing more than prove, that animals were killed, and flame extinguiſhed, in an exhauſted receiver. This indeed demonſtrated, that air was abſolutely neceſſary to animal life and combuſtion; but in what way it anſwered theſe great purpoſes, continued as great a myſtery as before. It remained, too, to be ſhewn, that only one part of the air entered the blood, during reſpiration, and was conſumed by combuſtion.—Here it was that Mayow, who was cotemporary with Boyle, but when Boyle was far advanced in years, commenced our knowledge of pneumatic chemiſtry. His invention of a pneumatic apparatus, the happy manner in which he executed his experiments, and his application of his diſcovery of vital air, to explain the wide influence of its effects in reſpiration and combuſtion, muſt give us a high idea of his genius. The works of this ingenious phyſician, certainly contain a great part of thoſe noble truths, which have given reputation to the names of Prieſtley, Scheele, Lavoiſier, and others. The reader, we truſt, will be convinced of this, from peruſing this publication, a conſiderable portion of which is taken up in examining the opinions of Mayow.

We are aware, that the attention of the medical world has been already directed to the works of this author, by two very eminent men, Dr. Scherer and Dr. Beddoes. The analyſis of the former is accurate, but deficient, as not containing the beſt and moſt noble of Mayow's diſcoveries: the extracts, too, are publiſhed, without pointing out Mayow's opinions, as they quadrate with thoſe of the moderns. Much laudable zeal is, however, diſplayed by Dr. Scherer, to reſtore to Mayow his long loſt honours; but the analyſis is incomplete, and does not impreſs the mind of the reader with the extent of Mayow's diſcoveries. We have aſſerted, as the reader will ſee in the work, that Mayow was not encouraged, by the adoption of his doctrines, during his own life time. This is contrary to the opinion of Dr. Scherer, who ſays, the Royal Society of Arts in London, Henry Munday, Phyſician at Oxford, and ſome others, received and adopted his doctrine, with pleaſure. An account of Mayow's experiments was, indeed, given in the Phil. Tr. previous to their publication; but, ſays Dr. Beddoes, the ſullen reviewer in the Tranſactions, could never once prevail on himſelf to ſmile upon the fair features of this new-born offspring of ſcience. I know not, upon what authority Dr. Scherer attributes the adoption of Mayow's opinions to Munday. I can ſay, from a peruſal of this Author's work, that he does not mention him*. The ingenious Dr. Beddoes was the firſt, who held up the merit of Mayow to the literary world. The abſtract, however, which he publiſhed, was rather haſtily done. He wiſhed only to let the world know, that there was ſuch a book in exiſtence, which merited the attention of the literary. His profeſſional, as well as other important avocations, hurried him too much, in detailing the beauties of Mayow; and, indeed, the ingenious Dr. Beddoes, with a candour and liberality peculiar to himſelf, has written me, that he is ſenſible his extracts from Mayow have been publiſhed in too curſory a manner *.

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* Βιοχρηστολογια, ſeu Commentarii, de Aere Vitali de Eſculentis, de Potulentis cum Corollario de Parergis in victu; Auct. Hen. Munday, Med. Oxon. 1680. I heard of this work in town. I knew, from conſulting Wood's Athenæ, that

Notwithſtanding the opinions of Mayow did not meet with the immediate reception which a candid examination would have produced, yet he was afterwards taken notice of, by ſeveral authors of credit and reſpectability. Many, however, by not under-

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that he was cotemporary with Mayow. I, therefore, went immediately to Oxford, to examine the Bodleian Library, with full hopes of finding an adoption and explanation of Mayow's opinions. I was, however, miſerably diſappointed, not only in this, but in not diſcovering any thing worth attending to. Appendix No. VI.

* Appendix No. I. and III.

underſtanding him, rejected his opinions; and others attributed to him doctrines, which he never held. Jocher, in his Lexicon, ſays, that Mayow's opinions did not meet with approbation; Dr. Beddoes imagines, that he aſſerts this upon the authority of Morhof. In the Polyhiſtor. no notice is taken, of what reception the nitro-aerial doctrine met with, in Mayow's own time: but he is repeatedly quoted by Morhof, on different ſubjects, and his opinions adopted*.

It is not neceſſary to ſay much about the nature of the following work; the table of contents ſufficiently ſhew that†. It was undertaken with a view of examining, how far the recent publications on chemiſtry and phyſiology, are entitled to the claims of novelty. I have long accuſtomed

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* Dr. Beddoes ſays that he wiſhed to ſee Morhof's account of Mayow, but unfortunately the Polyhiſtor had no index. The edition in his poſſeſſion muſt be very imperfect, for the one I was fortunate enough to procure in London has a very copious one.

† It was originally the intention of the author, to have given a large and full table of contents; but he found, that it would unneceſſarily increaſe the ſize of the work.

myſelf to, and been peculiarly delighted with, reading the works of the laſt century; an age diſtinguiſhed for the production of many learned men, in every department of ſcience. I have often ſtole an hour from my more immediate profeſſional engagements, that I might dedicate it to this ſort of reading. Little did I imagine, at firſt, that ſuch employment would be the ſubject of publication; and as little did I conceive, that I ſhould trace out facts, ſuppoſed to be but very recently diſcovered. I could not be ſurpriſed, too, when I obſerved theories, founded upon theſe facts, very ſimilar to thoſe we now entertain. I mentioned them, during converſation, in the philoſophical circles which I had the pleaſure to attend, during my reſidence in London. Some believed, more diſbelieved, and many ſuppoſed, that from the glow of converſation, exaggeration might creep in. Having collected a ſufficient number of facts, to juſtify my obſervations, I imagined they would afford an hour of entertainment to the curious reader.

It is a ſource, too, of rational and agreeable amuſement to the cultivated mind, to examine the ſimilarity of opinions, in diſtant periods, and to trace the progreſs of ſcience. It is curious to obſerve the co-incidence, that ſometimes take place, between the philoſophical ſyſtems of great men. As reſearches into nature ever afford a pleaſing employment to rational minds, and as her operations are all directed by fixed and immutable laws, it is not inconſiſtent to expect, that even in periods the moſt diſtant from each other, the philoſophical purſuits of the literary will converge to the ſame point. The ſame obſervations, with reſpect to facts, have, no doubt, repeatedly occurred to every enquirer; and reſults, which ariſe from the conſideration of general principles, have preſented themſelves, in a ſimilar point of view, to the examination of philoſophers. But it is not a looſe hint thrown out, nor a ſyſtem founded upon conjectural philoſophy, that entitles a man to the merit of a diſcoverer. An arrangement of facts, the reſult of experimental enquiry and cloſe reaſoning, alone, claims that honourable appellation. Laborious, indeed, is the inveſtigation, which requires it, and tedious the courſe. How carefully, therefore, ought we to avoid wreſting from the brow of a diſcoverer, the dear-bought crown of literary fame. It affords a grateful pleaſure to a feeling mind, to obſerve each man, poſſeſſing the rewards due to his labours, and however difficult it is, perhaps impoſſible, to award the *ſuum cuique;* yet inveſtigation, under almoſt every circumſtance, contributes to that end, and promotes the diſpenſation of diſtributive juſtice.

OBSERVATIONS, &c.

PART THE FIRST.

SECT. I.

OF FIRE-AIR PARTICLES.

I SHALL now enter into the principal object of this Publication, which is to ſhew, that what many of the moderns have given out to the world as their own, may be clearly traced in earlier authors. I ſhall begin with Mayow, the great chemical luminary of the laſt century, whoſe works every where abound with the moſt obvious traces of genius and ability. His publication commences with the hiſtory of nitre. He was aware, he ſays, that many authors had written upon this ſubject, as if it had been ordained, that nitre ſhould make as much noiſe in philoſophy as in war; but the truth, obſerves Mayow, ſeems ſtill obſcured, notwithſtanding the multitude of writers; and the properties of nitre are ſtill concealed from our knowledge.* When the wonderful phenomena produced by the combuſtion of gun-powder became more generally known, they would certainly arreſt the attention of the philoſopher; and it is probable, the chemiſt would more particularly direct his enquiry to the properties of nitre; but nothing ſatisfactory could be known on this ſubject till its elementary compoſition was detected.† On this ſubject, therefore, did our ingenious author employ his time; and upon the reſult of his beautiful experiments with nitre, were all his diſcoveries founded. The happy manner in which he conceived all his experiments, and the neatneſs and accuracy with which he ſeems to have executed them, could not fail of producing the moſt unequivocal reſults. It is evident, he ſays, from analyſis and ſyntheſis, that nitre is compoſed of an alkaline ſalt and an acid, the former derived from the earth, the latter, in part, from the atmoſphere: for if we diſtil nitre, the acid ſpirit will come over into the receiver, the alkali remaining in the retort; and by adding nitrous acid to an alkali, nitre will be formed. Mayow did not reſt ſatisfied with this rough mode of explanation; he was determined to diſcover the component parts of each, and examine what gave activity to the nitrous acid. This quality, he proves, is derived from the atmoſphere, and is the common principle

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* In the year 1670, four years before Mayow publiſhed the Collection of his Tracts, a Sir William Clarke, Phyſician to the King, publiſhed a book on Nitre, entitled, A Natural Hiſtory of Nitre, or a Philoſophical Diſcourſe on the Nature, Production, Situation, Artificial Extraction, Virtues, and Uſe of Nitre. Morhof ſays, it is a trifling work, *verum parum præſtitit pro dignitate hujus ſubjecti; nam in naturâ ejus indagandâ* exilis *admodum ipſius opera eſt.* The great Lord Verulam had directed the attention of the phyſician to nitre, by maintaining, in his Hiſtoria Vitæ et Mortis, that its daily uſe would prolong life.

† The diſcovery of gunpowder is commonly attributed to Barth. Schwartz, a German Jeſuit; but it is clear, that Roger Bacon, who lived ſome ages before, was the original diſcoverer.

of

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diſcoverer. It is a curious circumſtance, that both Morhof and Boerhaave give to Bacon the honour of the diſcovery, whereas Cambden attributes it to the Jeſuit. Vide Boerhaave's Chemiſtry, Shaw's Edit. p. 130.—Morhof's Polyhiſt. p. 422.

of acidity, and procured from the ſame ſource. In order that his ideas on this ſubject may be more clearly underſtood, he explains his theory of combuſtion, in which the reader will find a beautiful doctrine, and perfectly analogous to that of the modern diſcoverers. (A) In the firſt place, I think it will be granted, that an aerial ſomething, whatever it may be, is abſolutely neceſſary to the production of flame. The experiments of Boyle place this beyond a doubt;* for it appears from theſe, that a lighted candle is much ſooner extinguiſhed *in vacuo*, than when confined under a glaſs filled with air: an evident proof that the flame is not choaked, as ſome imagine, by the ſmoke, but goes out for the want of an aerial ſupply. He adds, (B) Beſides, any combuſtible body placed *in vacuo*, cannot be inflamed by burning coals, ignited iron, nor by the ſun's rays collected in the focus of a lens; ſo that we muſt allow, that certain aerial particles are neceſſary to the production of fire. Having premiſed this, he

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[*] Theſe experiments are related by Boyle, in his Nova Experimenta Phyſico Mechanica, de vi aeris elaſtica, et ejuſdem effectibus.

he goes on to obſerve, (C) But we are by no means to imagine, that the igneo-aerial ſupply is the whole air employed, but its more active and ſubtile part; for a candle under a glaſs will ceaſe to burn, although a conſiderable quantity of air remain. Having made theſe obſervations, which not only the experiments of Boyle, but alſo more recent inveſtigations have ſhewn to be perfectly agreeable to truth, he proves, by an experiment the moſt deciſive and convincing, that the active and igneous part of the air exiſts in nitre; (D) For we muſt obſerve, that nitre mixed up with ſulphur, will burn perfectly well, either *in vacuo* or under water, as is evident from the following experiment.

Let well levigated gun-powder be reduced into a pretty firm maſs, with a little water; with this fill a tube, the one end of which muſt be cloſed, ramming it tight with a piſton; then ſet the powder on fire at the open extremity of the tube; then invert it, and plunge it into water; in this ſituation the powder will completely burn away. The ſame powder, thus managed, burns equally well *in vacuo*, when all other burning bodies are extinguiſhed, for the want of the aerial ſupply; a clear proof that nitre contains the igneo-aerial particles, ſo neceſſary to combuſtion; hence there was no occaſion to ſupply them from the atmoſphere.*

That the phenomena of combuſtion are produced by the fire-air particles preſent in nitre, and not from any ſulphureous (phlogiſtic) matter, is proved by a variety of means. It is evident, that fire-air particles exiſt in nitre from its deflagration, when theſe particles are rapidly thrown off; for I cannot agree with Willis, that any ſulphureous matter exiſts in nitre.† For although the fire-air particles are abſolutely neceſſary to combuſtion, yet of themſelves they are not capable of inflammation, unleſs ſome combuſtible ſubſtance be added. (E) To the inflammation of any combuſtible matter, it is neceſſary that fire-air particles ſhould be ſupplied from the atmoſphere, or from nitre mixed with it; hence the reaſon why ſulphur will not burn *in vacuo* without nitre. On the contrary, there is no neceſſity that fire-air ſhould be ſupplied from the air for the combuſtion of nitre, as it will deflagrate ſufficiently well *in vacuo;* but for its inflammation, it is abſolutely neceſſary that ſome combuſtible body be mixed with it. If nitre too be thrown into an ignited crucible, it will not inflame; nor will this effect be produced by the flame of a candle, or rays of the ſun, except ſome combuſtible body be mixed with it, a clear proof that nitre contains no ſulphureous matter whatever: and hence it follows, that flame produced by combuſtion with nitre, ariſes from its fire-

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* Vide Mem. de Mathematique, &c. XVI. 686—1786. 4to. for an experiment of Lavoiſier's, very ſimilar to this. Bernoulli, in the laſt century, aſcertained the development of air by the deflagration of gun-powder. He placed *four* grains of powder in a recurved tube of glaſs, plunged the tube in water, and ſet fire to the powder by means of a burning glaſs; after the combuſtion, the interior air occupied a larger ſpace, ſo that the ſpace abandoned by the water, was ſuch as would have contained *two hundred grain* of gun-powder.—Hiſt. de l'Academie des Sciences de Paris, 1696, tome ii.—Memoire de M. Varignon ſur le Feu & la Flamme.

† Quod ſulphur copioſè inſit nitro, præter deflagrationem ejus, ſatis teſtatur ipſius geneſis; naſcitur enim præcipue

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cipue in locis ubi ſcatent animalium excrementa ſulphurea.—De Ferment. p. 77.

air particles. (F) This is the reaſon why the flame of nitre differs ſo greatly from that which is excited in any burning body; for the combuſtible body burns in conſequence of the fire-air particles derived from the atmoſphere, but nitre inflames on account of the fire-air particles thickly condenſed in itſelf: hence it happens, that the flame of nitre is ſo very violent.

Having thus taken notice that combuſtible matter will not burn *in vacuo*, except nitre be preſent, and that flame is extinguiſhed under a glaſs, although a conſiderable quantity of air remain, he denominates that part of the atmoſphere which ſupports combuſtion, Fire-air Particles*. Since alſo theſe fire-air particles exiſt in nitre, in common with the atmoſphere, he calls them alſo by the compound appellation of Nitro-aerial Particles, and Nitro-aerial Spirit.* He maintains, that the nitrous acid is a compound containing fire-air particles; but notwithſtanding this, it will not produce inflammation, becauſe theſe particles are ſurrounded with moiſture. Had he not been hurried on by an ardour of mind, conſcious of the truth of his doctrines, he might have been led to the ſtriking experiment of mixing nitrous acid with oil; for he himſelf afterwards ſhews, that oils, fat, &c. are principally compoſed of combuſtible matter. Mayow attributes heat too to the motion of fire-air particles. The doctrine that motion is the precauſe of heat muſt have been the firſt that preſented itſelf to thoſe who conſidered the ſubject. The common experiment of friction, the motion

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* The coincidence between this term of Mayow, and that of the Swediſh Chymiſt, is very remarkable. Mr. Scheele having collected ſome air from nitre, mixed it with three parts of air, in which fire would not burn, and it afforded an air ſimilar to the common ſort. He obſerves, ‘ Since this ‘ air is abſolutely neceſſary for the generation of fire, and ‘ makes one-third of our common air, I ſhall henceforth ‘ call it, for ſhortneſs ſake, Fire-air.’—Experiments on Air and Fire, p. 35. There is no foundation, ſays Dr. Beddoes, for the vague poetical term *empyreal*, the compound word *feuer-*

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feuer-luſt ſignifying preciſely fire-air.—Letter to Dr. Goodwin, p. 28.

* We ſee here Mayow forming a nomenclature for himſelf, the juſt reſult of his experiments, and which preſented itſelf to him from the new facts which occurred in the proſecution of his ſubject.

tion and extrication of heat, which took place in the union of different ſalts, &c. naturally produced this idea in the mind. The genius of Black had not yet explained the doctrine of latent heat, and Mayow was not aware of *that* which cauſed the ſolid and aeriform ſtate of bodies. This was, therefore, the moſt obvious theory that could occur to him, when he obſerved motion in all his experiments in which caloric was diſengaged: hence he adds, the reaſon why combuſtible bodies are as neceſſary to produce the phænomena of combuſtion is, becauſe they are fitted to ſtir up a very rapid and igneous motion with the nitro-aerial particles. We ſhould ſay, in modern times, that the oxygen, under a certain temperature, acquires a greater affinity for the elementary principles of the combuſtible body; hence it gives out heat and light. Mayow therefore attributed the phænomena of combuſtion to the motion, the moderns to the condenſation of fire-air. He explains himſelf more clearly afterwards. Having mentioned theſe properties of his nitro-aerial particles, he further examines into combuſtion and heat, as ariſing from them. He obſerves, (G) If we ſeriouſly attend to the nature of flame, and carefully examine what change the igneous particles are undergoing, while they are in a ſtate of inflammation, we ſhall conclude, that the inflammation of the igneous particles is produced in no other way, than by their very rapid motion. To prove that the combuſtible, and not the nitro-aerial particles are ignited, and alſo that both are neceſſary to the production of flame, he adduces another experiment. (H) If nitre be thrown into an ignited crucible, it will preſently be melted, but will not take fire; but if oil be injected into this crucible it will immediately inflame. That the igneous particles, collected in the ignited crucible, are of a nitro-ſaline nature (i. e. contain nitro-aerial particles) is evident, becauſe any combuſtible matter injected into this ignited crucible, immediately burſts into a flame; but combuſtible particles are by no means thrown into that rapid and igneous motion, except through the medium of nitro-aerial particles. Here it may be obſerved, our ingenious author makes no diſtinction between heat and fire-air. He ſaw the crucible ignited; ignition he maintained was owing to the motion of fire-air; any combuſtible ſubſtance, therefore, thrown into the crucible, would inflame, in conſequence of motion being communicated to it by this air.* Beſides, he obſerves, that particles of a combuſtible nature, are too groſs to penetrate ſolid bodies, as iron, glaſs, &c. for if a poliſhed plate of metal be held for a while in the flame of a candle, it will be penetrated and heated by the igneous particles; but that theſe are fire-air, and not combuſtible particles, is evident, becauſe the latter adhere to the ſurface of the metal, in the form of ſoot, and by no means enter it. In order to form the ſubtile and penetrating nature of his fire-air particles, he mentions a very remarkable fact, and till

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[*] Dr. Pearſon, the ingenious lecturer in chemiſtry, thinks that the blue appearance at the bottom of the flame of a candle, is owing to the combuſtion of hydrogen and carbon. Mayow thinks, that the blue flame, which is obſerved during the combuſtion of ſulphur, is owing to the leſs rapid motion of fire-air particles. In burning phoſphorus under a bell-glaſs, I have frequently obſerved, that at firſt, when the combuſtion is *very rapid*, the flame is *yellow*, but as ſoon as the inflamation begins to decreaſe in violence, the oxygen being *more ſlowly* applied, the flame becomes blue.

till of late not at all underſtood. Having obſerved that antimony is calcined in the focus of a burning glaſs, as if nitrous acid had been poured upon it, or as if it had been expoſed to the flame of nitre, for in this the fire-air particles are thickly condenſed, he emphatically adds, (¹)I muſt by no means omit to obſerve, that antimony calcined in the focus of a lens, is not a little increaſed in weight, as is known from experiment; nor can we conceive whence that increaſe of weight is derived, except from the *fixation of fire-air particles*, during the calcination. In all theſe proceſſes too, it acquires equally a diaphoretic quality. Here is an obſervation, that medicines act as explained by the modern phyſiologiſt, and we know it to be conformable to truth.

Dr. Thornton has informed me, that the breathing of oxygen, produces an increaſed exhalation from the ſkin; and indeed that gentleman, with a liberality which ſhould every where be inſeparable from the Medical Profeſſion, has permitted me to be preſent when he receives his patients, and to aſk queſtions, with reſpect to the operation of pneumatic remedies.—From every enquiry I have made, I have found, that an atmoſphere of a higher than ordinary ſtandard promotes the cuticular diſcharge. The additional weight acquired during oxydation, is, by Mayow, attributed to the right cauſe. How has this excellent obſervation been thrown away upon the Stahlians? How has not philoſophy been tortured to explain the contradiction of increaſed weight with diſengaged phlogiſton? How have they not been obliged to recur to the imaginary cauſe of repulſive power and principle of liberty, by neglecting the obvious and ſimple explanation of Mayow? They ought to have recollected, too, the rules of the great Lord Verulam, the firſt who rejected hypotheſes not founded on direct experiment, that to reaſon from occult cauſes, is an inexhauſtible ſource of error. From what has been here delivered, it is evident that Mayow met with oxygen, which he never, however, collected in a ſeparate ſtate, and from the manner of his expreſſions, we can have no doubt that he diſcovered that active principle, which he has ſo admirably employed in the explanation of his experiments. If the reader, however, ſhould be inclined to with-hold his aſſent, we think he will be convinced from what will be afterwards detailed; but before we proceed any further, it will be neceſſary to explain, what Mayow means by his Salino-ſulphureous particles, which are hereafter ſo often mentioned.

SECT. II.

OF SALINO-SULPHUREOUS PARTICLES.

From experiments made by different chemiſts, it appears, that oils, fats, and the food we eat, are compoſed principally of hydrogen, azot, and carbon. When hydrogen and carbon are combined together, without the intervention of caloric, to bring the hydrogen into the ſtate of gas, they form oil, which is either fixed or volatile, according to the proportions of hydrogen and carbon in its compoſition. By means of calculation applied to the products of experiments, we find, that fixed air is compoſed of twenty-one parts, by weight, of hydrogen, combined with ſeventy-nine parts of carbon. Animal ſubſtances, being compoſed of nearly the ſame elements with cruciferous plants, give almoſt the ſame products in diſtillation; with this difference, that, as they contain a greater quantity of hydrogen and azot, they produce more oil and more ammoniac*. It is from theſe elements of hydrogen, azot, and carbon, by different combinations, that various ſubſtances, apparently very diſſimilar, are formed. It is by theſe that the animated creation has its life and being. From theſe we derive our daily nouriſhment, and by their union with oxygen, taken in by the lungs, animal heat is diſengaged, the chief ſtimulus to the irritability of the animated fibre. Again it is by the union of theſe with oxygen, that combuſtion is ſupported. We ſhall ſee ſomething very like this in Mayow. By the by, I muſt obſerve, that he uſes ſulphureous and ſalino-ſulphureous, indiſcriminately, to expreſs the ſame thing. The word ſulphur was one of the moſt vague terms made uſe of by the older chemiſts. It was applied to the inflammable principle of bodies, and all ſubſtances capable of inflammation, were ſaid to be ſulphureous; hence oils, reſins, animal fat, &c. were by ſome called ſulphurs. Sulphur, however, properly ſo called,

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* Elements of Chemiſtry by Mr. Lavoiſier, p. 165-7-82. 3d Edit. by Kerr.

was ſuppoſed to be a compound, conſiſting of concentrated oil of vitriol and phlogiſton*; and ſulphur, being a very inflammable ſubſtance, the terms phlogiſtic and ſulphureous became ſynonimous. Whenever, therefore, Mayow uſes the terms ſulphur, ſulphureous, and ſalino-ſulphureous particles, he means ſimply combuſtible matter, capable of producing flame, by a rapid motion with fire-air. We wiſh the reader to recollect this, whenever he meets with theſe terms. He ſays that the fat of animals is formed of theſe ſulphureous particles; we may learn, from a variety of expreſſions, that this was his opinion, ſuch as the ſulphureous particles of the blood, of which the fat is compoſed, ſulphureous particles producing fat, &c. (K) Our food, he maintains, is principally compoſed of ſuch matter; moreover our nouriſhment is replete with ſalino-ſulphureous particles: hence thoſe ſubſtances which copiouſly contain a volatile ſalt and ſulphur, are beſt adapted to recruit the ſtrength worn out by daily labour*. The following paſſage from Mr. Hewſon, ſhews how accurately Mayow had attended to the animal œconomy; ſpeaking of the abſorption of fat for nouriſhment, he adds, 'This circumſtance was clearly proved by my valuable and ingenious friend, the late Dr. Stark, who, in a courſe of curious experiments made by weighing himſelf, after living for ſome time on different ſorts of food, diſcovered that a leſs quantity of ſuet was ſufficient to make up for the waſte of his body, than of any other ſort of ordinary food; and that when compared with lean part of meat, its nutritive power was, at leaſt, as three to one'†. If any one

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* Vide Neumann's Chemical Works, p. 165;—and Lewis's Mat. Med. p. 559.

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* We know, ſays Mr. Lavoiſier, that animal ſubſtances are compoſed of hydrogen, carbon, azot, phoſphorus, and *ſulphur*, all of which, in a ſtate of quintuple combination, are brought to a ſtate of oxyd, by a larger or ſmaller quantity of oxygen.—Elem. of Chem. p. 202.

† Experimental Enquiry into the Properties of the Blood, p. 127-8. The ingenious Dr. Stark, at an early age, fell a victim to his zeal for experimental enquiry, his death having been occaſioned by a courſe of experiments (made upon himſelf) on the effects of various kinds of food. Vide Med. Communications, Vol. I. art. 24.

ſhould ſtill doubt that theſe expreſſions, made uſe of by Mayow, were merely vague and looſe terms, thrown out without an expreſs intention, the following paſſage, in which he mentions, ſtill further, what ſubſtances contain the ſalino-ſulphureous particles, muſt convince, that he attached a preciſe idea to theſe terms; for he obſerves, that *ſpirit of wine*, and the chemical *oil* of vegetables, are fluids replete with a volatile ſulphur. It was thought neceſſary to premiſe theſe obſervations, that the reader might not imagine, the expreſſions of Mayow were tortured to a different meaning, when it is ſaid, that his ſalino-ſulphureous particles are what we now find to be the compoſition of different ſubſtances, and to which the French chemiſts have given the appellation of hydrogen, azot, and carbon. We do not mean to ſay, that Mayow *exactly* meant this; or that he maintained his ſulphureous ſubſtances were compoſed of different elementary principles; but when he mentions the ſubſtances, containing the ſalino-ſulphureous particles, the component parts of which are now known to be hydrogen, azot, and carbon, we may fairly conclude that the chemical agency which Mayow aſcribes to the former, is analogous to that of the latter; and that certain principles, acting in theſe ſubſtances, produced chemical effects, during their operation, ſimilar to thoſe now attributed to the elements of the more combuſtible bodies. All ſebaceous ſubſtances, indeed, were, by Stahl and his followers, termed phlogiſtic, as containing the principle of inflammability, which continued in uſe, till the late happy revolution in chemiſtry, by detecting the ambiguity of ſuch expreſſions, has given to ſubſtances appropriate names.

SECT. III.

Of Acids.

On the conſtitution of the acids, the ingenious Mayow is no leſs curious; and we diſcover a ſtriking analogy between his facts and reaſonings, and thoſe of the modern chemiſts, on this ſubject. The reader will, probably, be ſurpriſed to find the following account of the formation of the acids, in ſo old a writer; for it is exactly ſimilar to the explanation which the new diſcoveries have given riſe to. He has already, as we have ſeen, deſcribed, that active principle in the atmoſphere, which forms a part of the nitrous acid; he now wiſhes to account for its conſtituents: but before he does this, it is neceſſary to lay down a general theory of the acids, for, as he obſerves, there is a great ſimilarity among all of them. He rejects the opinion hitherto entertained, that an acid exiſts, ready formed, in ſulphur, becauſe in its union with alkalis, no efferveſcence takes place, and we cannot detect it by any chemical means. The ſulphuric acid, he ſays, is formed by the union of fire-air particles with the ſulphur, during combuſtion. He ſuppoſed ſulphur to contain particles of an alkaline nature, and the following idea which he gives, of the manner in which the fire-air particles unite with the ſulphur, in no wiſe effects the truth of his theory, as it is a mode of expreſſion which he borrowed from the language of the times. (L) During the burning of ſulphur (*i. e.* whilſt the ſulphureous particles and fire-air are hurried into a motion conſtituting fire) the ſaline particles, adhering to the ſulphureous, are broken down, and comminuted by the repeated ſtrokes of the fire-air, ſo that theſe ſaline particles being frequently bruiſed and broken down, are ſo attenuated, that inſtead of being firm and ſolid, they become flexile and fluid. Moreover the ſaline particles of the ſulphur, which were before of a fixed nature, after they become ſharpened and reduced to fluidity, are converted into a ſharp and ſour liquor, and form, as is very probable, the common ſpirit of ſulphur*. The acids of woods are formed in the ſame way, as are alſo the acids of ſugar and honey†. His explanation of the formation of vitriol from martial pyrites (ſulphuret of iron), ſhews how juſtly he conceived this ſubject. (M) For indeed the nitro-aerial ſpirit, efferveſcing with the metallic ſulphur of the marchaſites, converts their more fixed part into an acid, which immediately attacks and calls forth the metallic particles of the mineral, and unites with them into a vitriol. Moreover the ruſt of iron, which poſſeſſes the nature of vitriol, appears to be produced by the nitro-

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[*] This mechanical explanation has not loſt its effects in our days. Mr. Lavoiſier, when treating on the formation of gas, obſerves, ‘It is perhaps more natural to ſuppoſe, that the particles of caloric have a ſtronger mutual attraction, than thoſe of any other ſubſtance; and that theſe latter particles are *torn aſunder*, in conſequence of this ſuperior attraction of the particles of caloric, which *forces* them between the particles of other bodies, that they may be able to reunite with each other.’—Elements of Chemiſtry, p. 72.

[†] The acid of wood is now termed pyro-lignous; Mr. Goſtling was the firſt who made a particular enquiry into its nature and properties.—Vide Crell's Chemical Journal for 1779.

nitro-aerial particles, attaching to the metallic ſulphur of the iron. In this way, he obſerves, ruſt, or an imperfect vitriol, is produced, in the ſame manner as if an acid had been thrown upon the iron. Has he not here anticipated the modern chemiſts, in their ideas of an oxyde? The ſame principle which conſtitutes acidity, thus unites with metals, to form ruſt; for we muſt obſerve, he adds, that not only in ſolids, but alſo in fluids, an acid ſalt, or ſharpneſs, is produced, by the action of the nitro-aerial ſpirit. Of this he gives an example in the ſouring of wines, which owe their acidity to the union of nitro-aerial particles. (N) On this account, wines or ales, expoſed to the rays of the ſun, or kept warm, in proceſs of time are converted into vinegar; for the nitro-aerial particles being communicated to thoſe liquors, either from the ſolar rays, or the heat, (he imagined heat was owing to the rapid motion of nitro-aerial particles, and that they formed a part of the ſun's rays), effervеſce with their ſalino-ſulphureous particles; whence it happens, that theſe ſaline particles are acidified, by the action of the nitro-aerial

particles

particles and converted into acid ſalt*. Notwithſtanding, however, that there is a great ſimilarity between the acids, in conſequence of fire-air being the general principle of acidity in all, yet there is a difference obſervable in them. This is owing to the nature of the acidifiable baſe, and the degree of oxygenation of which it is capable†. Sugar, mucus, under which term are included the different kinds of gums and ſtarch, are vegetable oxydes, having different proportions of hydrogen and carbon, as their radicals or baſes. By the addition of

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[*] According to the new chemical doctrine, the acetous fermentation, or oxygenation of vegetable matters, takes place, in conſequence of the increaſed temperature altering the ſtate of affinity ; for the chemical attraction which exiſts between oxygen, and various ſubſtances, is varied, in proportion to the degree of temperature. Although Mayow, however, here mentions heat, as neceſſary to the acetous fermentation, he does not ſeem to be aware, that the increaſed temperature produced a change in the ſtate of chemical affinity.

[†] Thus the baſe may be double or triple, very rarely ſingle. The baſe may be compoſed of carbon and hydrogen, or of carbon, hydrogen, and phoſphorus, combined with ſome oxygen.

of a certain quantity of oxygen, they become acids; and according to the degrees of oxygenation, and the proportion of hydrogen and carbon in their baſes, they form the ſeveral kinds of vegetable acids*. (o) As to the difference of acid liquors, obſerves Mayow, we muſt conclude, that it is produced by the diverſity of the ſalts (baſe) of which they are compoſed, *viz.* that the fixed ſalts are ſometimes more, ſometimes leſs broken down and acidified by the nitro-aerial ſpirit. There is, nevertheleſs, a great ſimilarity between each of the acids; and the nitro-aerial and fire-air particles are contained in *all* of them, as in a proper ſubject.

Such is the conſiſtent and beautiful manner, in which this great genius applied his diſcovery of fire-air, to explain the formation of the acids. From his experiments and acute obſervation, he beheld the neceſſity of a principle to give activity to the acids, and without which there could be no acidity. It occurred to him too, that this principle muſt be united to ſome baſe, before acidity could be formed. Mayow never reſted with a ſimple fact: it was his ruling paſſion, to explain and come at the elementary principles of every thing. He might have been content with ſaying, that nitre was formed of an acid and an alkali:—this did not ſatisfy him, he muſt give the conſtituent principles of nitre. We know that nitre is formed of three ſubſtances, *viz.* fixed alkali*, azot, and oxygen. Mayow ſaw from his experiments, that there muſt be a triple combination in the formation of nitre. This ſalt, he ſays, is compoſed of three principles, *viz.* 1*ſt*, of an active part, derived from the atmoſphere; 2*d*, this uniting to the ſaline vehicle, conſtitutes the nitrous acid; which, 3*d*, attaching the fixed ſalts of the earth, forms nitre.

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* Lavoiſier's Elements of Chemiſtry, p. 172.

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* The fixed alkalis have never yet been decompoſed; and we are ſtill ignorant, whether they be ſimple or compound bodies. The conſtituent parts, however, of the volatile alkali, or ammoniac, have been diſcovered by Mr. Berthollet, to be azot and hydrogen.—Vide Memoirs of the Academy, for 1784, p. 316. Analogy has led Mr. Lavoiſier to imagine, that all the alkalis are compound bodies, and that azot is one of their elementary principles; but from ſome experiments related in the Tranſactions of the Turin Academy, there is reaſon for ſuppoſing, that ſoda is a modification of magneſia, and this latter ſubſtance, according to the experiments of Baron Born, appears to be a metallic oxyd.

The principle, which thus conſtitutes the moſt active part of the atmoſphere, gives acidity to ſubſtances, and is the ſupport of life and flame, is alſo the chief agent in fermentation. (P) The expreſſed juice of vegetables, as the muſt of wine and cyder, &c. effervesce, by the nitro-aerial and ſalino-ſulphureous particles inherent in them; or the nitro-aerial particles may be ſupplied, *ab extra*. He thinks it very probable, that the putrefaction, as well as the acetous fermentation, is produced by the nitro-aerial particles: (Q) Hence thoſe things which exclude the nitro-aerial particles, preſerve bodies from corruption; and this is the reaſon, why the fruits of vegetables, as alſo fleſh covered with butter, are long kept from putrefaction, and alſo iron, beſmeared with oil, is not corroded by ruſt.

We know, from the experiments of Pringle and Macbride, that carbonic acid is highly antiſeptic, and that various other ſubſtances poſſeſs this quality; and ſome curious experiments, made by

Dr. Alexander, prove, that meat, expoſed to the exhalations of a common privy, is prevented from undergoing the putrefactive proceſs for a long time, which, as he maintains, was certainly owing to the diſengaged ammoniac. How far theſe ſubſtances act by excluding the fire-air particles, or oxygen, remains for future inveſtigation. One experiment, however, which we made, proves, that if oxygen does not accelerate, at leaſt it does not retard putrefaction. I took three pieces of raw beef, of nearly the ſame ſize. One was confined with oxygen, the ſecond with hydrogen, and the third remained expoſed to the atmoſpheric air, for a ſtandard. After remaining for five days, the pieces of meat were taken out, extremely fœtid. The one in oxygen felt firm, and was of a bright red colour throughout; the other was very lax and dark-coloured, particularly at its upper ſurface, where it had been in contact with the hydrogen, the under part being ſomewhat redder. The ſtandard piece was not putrid; it had become perfectly dry: whereas thoſe in the glaſs were kept moiſt, by the confined evaporation from the water; this no doubt produced its effect. The experiment was performed in the month of April, 1796; the thermometer in the room varied from 40 to 48. We have not had ſufficient time to continue experiments of this kind; but we are juſt about commencing experiments, to diſcover, how far the different gaſes accelerate or retard the vinous, acetous, and putrefactive fermentations.

(A) Primo, concedendum eſſe arbitror, nonnihil, quicquid ſit, aereum, ad flammam quamcunque conflandam neceſſarium eſſe; id quod experimenta Boyliana, extra dubium, poſuerunt; utpote, ex quibus conſtat, lucernam accenſam multo citius in vitro aere vacuo, quam in eodem aere repleto expirare; indicio manifeſto, flammam vitro incluſam, non tam a proprio fuligine, uti nonnullis viſum eſt, ſuffocatam, quam pabulo aereo deſtitutam interire*. P. 11.

(B) Præterea, materia quævis ſulphurea in vitro, ex quo aer exhauſtus eſt, collocata neque carbone,

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[*] Tractatus Quinque Medico-Phyſici, ſtudio Joh: Mayow, LL. D. et Medici, necnon, Coll. Omn. Anim. in Univ. Oxon. Socii.—An. Dom. 1674.

carbone, aut ferro ignitis, neque radiis ſolaribus, ſpeculi uſtorii ope collectis, accendi poteſt; ut minime jam dubitandum ſit, quin particulæ quædam aereæ, ad ignem excitandum omnino requiruntur. P. 12.

(C) At non eſt exiſtimandum, pabulum igneo-aereum ipſum aerem eſſe, ſed tantum partem ejus magis activam ſubtilemque; quippe lucerna, vitro incluſa, expirat, cum tamen copia aeris, ſatis ampla, in eodem continetur. P. 12.

(D) Quippe annotare eſt, nitrum ſulphuri admixtum, in vitro aere vacuo, item ſubter aquas, ſatis prompte deflagrare; uti ſequenti experimento conſtabit.—Nempe pulvis pyrius, minutiſſime tritus cum aquæ tantillo, in maſſam duriuſculam redigatur, quâ tubulus quivis, cujus altera extremitas obturata eſt, denſe impleatur, materiam ſubinde cum bacillo fortiter impellendo; dein pulvis iſte pyrius in extremitate tubuli apertâ accendatur, tubuluſque inverſus aquæ immergatur, ibidemque detineatur; ita pulvis pyrius, ad totalem ſui abſumptionem, ſubter aquas deflagrabit. Quinetiam pulvis iſte, modo prædicto diſpoſitus, in vitro aere vacuo, ardebit, cum tamen ignes alii ob pabulum aeris ſubductum mox extinguuntur: indicio ſatis manifeſto, ſal nitrum particulas igneo-aereas, ad flammam conflandam requiſitas, in ſe continere; ita ut ad ejus deflagrationem, particulas igneas ab aere ſuppeditari, minime opus ſit. P. 13.

(E) Ad materiæ cujuſque ſulphureæ accenſionem, requiritur, ut particulæ igneo aereæ ab aere, aut a nitro, ei prius admixto, ſuppeditentur: quæ cauſa eſt, quod ſulphur, in loco aere vacuo, flammam non concipiet, niſi nitrum ei admixtum fuerit. E contra, vero, ad nitri accenſionem non eſt opus, ut particulæ igneo-aereæ ab extra ſuggerantur, utpote quod in locis, a quibus aer præcluditur, ſatis prompte deflagrabit; verum, ad ejus accenſionem, omnino requiritur, ut materia aliqua ſulphurea ei admiſceatur. P. 15.

(F) Et hinc eſt, quod flamma nitri valde diverſa ſit ab ea, que a materia quavis ſulphurea deflagrante, excitatur; quippe materia ſulphurea particulis igneo-aereis ab aere ſuggeſtis, nitrum autem particulis igneo-aereis, in ipſo confertim agglomeratis, agmineque denſiſſimo erumpentibus deflagrat; unde ſit, quod flamma nitri maxime impetuoſa ſit. P, 16.

(G) Si ad flammæ naturam ſerio attendamus, et nobiſcum cogitemus, qualem demum mutationem particulæ igneæ ſubeunt, dum eœdem accenduntur, nihil aliud certe concipere poſſumus, quam particularum ignearum accenſionem, in motu earum perniciſſimo, conſiſtere. P. 25.

(H) Si nitrum in crucibulum ignitum immittatur, idem mox liquabitur, non vero flammam concipiet; cum tamen oleum crucibulo dicto injectum, ſtatim accendetur. Quod vero particulæ igneæ, in crucibulo ignito agglomeratæ, indolis nitro-ſalinæ ſint, indicio eſt, quod materia quævis ſulphurea, crucibulo dicto injecta, a particulis iiſdem accenditur; particulæ autem ſulphureæ non, niſi particularum nitro-aerearum ope, in motum velociſſimum igneumque, ſuſcitantur. P. 26.

(I) Neque illud prætereundum eſt, quod antimonium, radiis ſolaribus calcinatum, haud parum in pondere augetur; uti experientiâ compertum eſt; quippe vix concipi poteſt unde augmentum illud antimonii, niſi a particulis nitro-aereis igneiſque ei, inter calcinandum infixis, procedat. P. 28.—9.

(K) Sed inſuper alimentum particulis ſalino-ſulphureis refertum; hinc ea, quæ ſale volatili ſulphure que copioſo conſtant, vires laboribus diuturnis attritas reficiendo, præcipue idonea ſunt. P. 30.

(L) In ſulphuris deflagratione (dum, viz. particulæ ejus ſulphureæ et nitro-aereæ mutuo ſe motu igneo exagitant) particulæ ſulphuris ſalinæ, particulis ejus ſulphureis adhærentes, crebris particularum nitro-aerearum ictibus, verberantur, atteruntur, comminuunturque; ita ut particulæ eæ ſalinæ, ſæpius attritæ et contuſæ, tandem inſtar gladiolorum exacuantur, et inſuper adeo attenuentur, ut eædem a rigidis ſolidiſque, in flexiles fluidaſque convertantur. Particulæ, vero, ſulphuris ſalinæ, quæ antea indolis fixæ fuerant, poſt quam ita exacuuntur, et ad fluorem perducuntur, in liquorem aereum acidumque convertuntur, ſpiritumque ſulphúris vulgarem, uti veriſimile eſt, conſtituunt. P. 34.

(M) Nimirum ſpiritus nitro-aereus, cum ſulphure metallico marchaſitarum iſtarum efferveſcens, partem earum fixiorem in liquorem acidum convertit, qui mox, ab ortu ſuo, particulas metallicas lapidis dicti adoritur evocatque, tandem-que cum iiſdem in vitriolum coaleſcit. P. 39—40.

(N) Huc etiam ſpectat, quod vina aut cereviſia generoſior, radiis ſolaribus diù expoſita, aut in loco calido detenta, proceſſu temporis in acetum commigrant: nempe particulæ nitro-aereæ, a radiis ſolaribus, aut ab igne, liquoribus iſtis communicatæ, (etenim alibi oſtenſurus ſum, calorem quemcunque a particulis nitro-aereis, in motu poſitis, provenire) cum particulis liquorum eorum ſalino-ſulphureis effervefcunt; unde fit, quod particulæ ſalinæ, particularum nitro-aerearum actione exacuantur, inque ſalia acida convertantur. P. 41.

(O) Quoad differentiam liquorum acidorum, eam a diverſitate ſalium, e quibus iidem conſtituuntur, procedere, putandum eſt, uti etiam ex eo, quod ſalia fixa nunc magis, nunc vero minus, a ſpiritu nitro-aereo, atterantur exacuanturque: et tamen inter ſalia acida quæcunque, affinitas magna eſt et ſimilitudo; inque iis omnibus particulæ nitro-aereæ igneæque, veluti in ſubjecto idoneo, hoſpitantur. P. 44.

(P) Succus ex vegetabilibus expreſſus, veluti muſtum vini, aut pomacei, idque genus alia, ope particularum nitro-aerearum ſulphurearumque ſibi innatarum, effervefcunt. P. 60.

(Q) Hinc ea, quæ ſpiritum nitro-aereum excludunt, res a corruptione vindicant; uti etiam carnes butyro coopertæ, a putredine diu præſerventur; item ferrum, oleo illitum, rubigine non corrodatur. P. 62.

SECT. IV.

Of the ANALOGY *between* RESPIRATION *and* COMBUSTION.

From what we have already extracted from the works of our ingenious Author, the reader will clearly perceive, that he was acquainted with an active principle, conſtantly preſent in the atmoſphere, and which was neceſſary to the maintenance of flame. Here he enters more deeply into the ſubject; and by a ſet of beautiful experiments, not only proves it beyond a doubt, but alſo unequivocally ſhews, that the ſame aerial matter is conſumed, both by reſpiration and combuſtion. Theſe experiments are inſtituted, chiefly, with a view of proving, that the elaſticity of atmoſpheric air, in part, depends upon the nitro-aerial particles.

(A) If a cupping glaſs be applied to the ſkin, a partial vacuum will be formed, as ſoon as the flame is extinguiſhed; hence the ſkin will riſe into the glaſs, on account of the preſſure of the ſurrounding air. What is the cauſe of this, ſays Mayow, or whence does it happen, that theſe circumſtances are produced? (B) It is very probable, that the air is cloſely united to the flame, being taken up by it, as a pabulum; ſo that there is not the ſmalleſt portion of flame, in which there does not exiſt ſome portion of air. Hence we may conclude, that the vacuum is formed, becauſe the air is deprived of its fire-air particles, by the combuſtion; as is more clearly proved by the following experiments.

(C) Let a lighted candle be ſo placed, in water, that the wick ſhall riſe about ſix fingers breadth above the ſurface; a glaſs veſſel is then to be inverted over it, as is ſhewn in *Fig.* 1. Care muſt be taken, at the ſame time, that the water be of the ſame level, within and without, by means of a ſyphon, the one leg of which is previouſly placed within the inverted glaſs: this will let out the air as it is compreſſed by the immerſion of the veſſel. The ſyphon is then to be withdrawn, leſt afterwards the external air ſhould ruſh in. In a ſhort time you will perceive, while the candle is yet burning, the water riſing into the inverted veſſel. He will not deny, but that the aſcent of the water may be, in part, owing to the included air being leſs rarified and agitated, at the moment the candle is going out. It is by no means owing to this cauſe alone; but it is produced by the combuſtion of the candle deſtroying the nitro-aerial and elaſtic particles of the air, ſo that it is no longer able to reſiſt the preſſure of the atmoſphere. Leſt any one ſhould with-hold his aſſent to the inference, drawn from the above experiment, he inſtitutes another, that no doubt may remain, as to the fact he wiſhes to eſtabliſh.

(D) Let a piece of camphor, with ſome tinder dipt in melted ſulphur, be ſuſpended in a large glaſs veſſel, inverted, as is ſeen in *Fig*. 1. The veſſel is to be immerſed, ten fingers breadth, in the water, which is to be brought to the ſame level, both within and without, by the means already deſcribed. That the view, however, may be more diſtinct, ſome water may be taken out of the external veſſel. Let the height of the water, in the inverted glaſs, be marked, by paſting bits of paper againſt its ſide. After theſe preparations, let the combuſtible matter be fired, by a burning lens. The water, at firſt, will be depreſſed by the rarefaction of the air. When the combuſtion, however, has ceaſed, and the apparatus cooled, you will find the water to have riſen above its priſtine height. After the ſmoke was diſſipated, he attempted to fire, by the lens, a portion of the camphor, which had not been deſtroyed by the combuſtion; but the experiment did not ſucceed: a ſufficient proof, that the air was deprived of its fire-air particles, by the combuſtion; ſo that it was no longer able to ſupport that proceſs. This was ſurely a rigorous experiment, and proved inconteſtibly, that the atmoſphere was compoſed of two kinds of air, the one fit to ſupport flame, the other intirely deſtructive of it. The diminution that took place, in conſequence of the abſorption and condenſation of the fire-air, made Mayow imagine, that the elaſticity of the air depended upon the preſence of that gas. He was unwilling, however, to yield implicit aſſent to this opinion, apparently founded upon fact, as we ſhall ſee in the ſequel. It was his opinion, as has been already

ready ſtated, that the ſame elaſtic, and fire-air particles, were ſubſervient to the purpoſes of reſpiration and combuſtion. To determine this point, the following experiments were made.

(E) Let a moiſtened bladder be tied over the mouth of a circular glaſs veſſel; then let another, containing a mouſe, be applied tight to the bladder, as is ſeen in *Fig.* 2. The edge of the veſſel will ſhortly be ſeen to adhere firmly to the bladder, and the bladder itſelf will riſe into the cavity of the glaſs, in the ſame manner as if a cupping glaſs had been uſed, with a burning body in it; and this will take place while the animal is breathing: hence an animal may be uſed, inſtead of flame, in the operation of cupping. From this it appears, that the air is deprived of its elaſticity by the reſpiration of animals. Not content, however, with merely ſhewing, that one part of the air is conſumed by reſpiration, he inſtitutes another experiment, to determine how much the air is diminiſhed.

(F) Let an animal, confined in a cage, be ſuſpended in a glaſs cucurbit, in a ſimilar manner as the ſmall jar is in *Fig.* 4. The cucurbit is then to be inverted over water, which is to be made to aſcend into the glaſs, by means of the ſyphon. Some of the external water is then to be taken out, that we may have a more diſtinct view; and the height of the water, within the glaſs, is then to be marked, by ſlips of paper affixed to its ſides. In a ſhort time you will ſee the water gradually riſing, within the glaſs, although it ſhould ſeem, that a contrary effect would be produced by the heat and exhalations from the animal: and from experiments made with various animals, I have diſcovered, that the air is diminiſhed one-fourteenth by reſpiration.

(G) From what has been ſhewn, it evidently appears, that the reſpiration of animals exhauſts the air of certain vital and elaſtic particles; ſo that we can no longer doubt, that ſomething from the air, abſolutely neceſſary to life, is taken into the blood by reſpiration. It is manifeſt too, that the air is deprived of its elaſtic power, by the reſpiration of animals, in a ſimilar manner as by combuſtion; and indeed it is rational to believe, that theſe two proceſſes deprive the air of the ſame particles, as the following experiment decidedly proves.

(H) Let an animal be encloſed, with a burning candle, in a glaſs, inverted over water. In a ſhort time, the candle will be extinguiſhed, nor will the animal long ſurvive; for I have found, that the animal will not live much above half the time that it would have done, had it been confined without the candle. Leſt any ſuſpicion ſhould be entertained, that the animal was ſuffocated by the fumes of the candle, he made the experiment with ſpirit of wine; the animal therefore could not have died from that cauſe. Since then the air included in the glaſs, is deprived, in part, of the nitro-aerial particles, by the burning of the candle, it could not be long of uſe to the reſpiration of the animal; hence not only the candle, but alſo the life of the animal, will be ſhortly extinguiſhed, from the defect of nitro-aerial particles. He ſays, the animal ſurvives the flame, becauſe the candle requires a continual and rapid ſupply of nitro-aerial particles, to ſupport combuſtion.

SECT. V.

Of the different Gaſes MAYOW *met with in his* EXPERIMENTS.

Before we proceed to lay before the reader, Mayow's application of the fire-air particles to the purpoſes of the animal œconomy, it may not be improper to enquire, how far he was acquainted with the other kinds of air. The reader muſt not expect to find accuracy of diſtinction between the airs, by Mayow; but he will perceive the ſame acuteneſs of obſervation, and quickneſs of thought. His mind being wholly engaged with the diſcovery of fire-air, and his thoughts entirely occupied with the wide extent of its influence, he did not pay much attention to the other gaſes which eſcaped, during his chemical operations. He, however, collected different kinds of unreſpirable airs, and proved by experiment, that they were unfit to ſupport life, and that the fire-air particles were alone adapted to that purpoſe. He had to invent, too, a method for transferring air, from one veſſel to another, which, like the egg of Columbus, appears ſimple and obvious, after it is explained; but before explanation, required ingenuity, thought, and reflection. To be ſenſible of the merit of the following contrivances, ſays Dr. Beddoes, we have only to recollect, how difficult it muſt have appeared, before a living philoſopher, of whom this country has juſt reaſon to be proud, a ſecond time taught us the art, to confine, divide, remove from veſſel to veſſel, examine, and manage at pleaſure, fugitive, incoercible, and impalpable fluids, like that which we breathe*. Mayow fully exhibits this ingenious invention, in his enquiry, and diſplays a mind, ardent, ſagacious, and obſervant.

(1) I attempted to fire, ſays Mayow, combuſtible matter, under a glaſs, in which an animal had died; but the experiment failed: therefore it is probable, that the ſame air which is unfit for reſpiration, will not ſupport combuſtion. Mayow learnt, from his experiments, that the air, deprived

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* Honorable H. Cavendiſh's Exp. on Factitious Air. Ph. Tranſ. lvi. p. 142 and the plate.

of fire-air particles, become lighter*; (K) For, he obſerves, if a mouſe, or a bird, be placed in the ſuperior part of the glaſs, it will die ſooner, and the water will not riſe ſo high, as if the animal had been ſituated lower. This is more manifeſt, if two animals be uſed; for the one at the bottom of the glaſs will ſurvive the one at the ſuperior part. (L) From theſe facts we may be allowed to imagine, that the air thrown out from the lungs becomes lighter, becauſe it is deprived of nitro-aerial particles, and therefore aſcends to the top of the glaſs. The ſame exactly happens with reſpect to a burning body; (M) For if a glaſs cucurbit be inverted, and ſuſpended in the air, and a candle be held up under it, it will, in a ſhort time, be extinguiſhed; for the air contained in the glaſs, being made unfit to ſupport flame, by the combuſtion of the candle, and being, by the ſame means, rendered

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[*] Oxygen is more ponderous than the air of the atmoſphere; the cubic foot of atmoſpherical air wighing 720 grains, while the cubic foot of purer air weighs 765—Chaptal's Elem. of Chem. Vol. p. 120. According to Mr. Kirwan, the weight of oxygen, to that of common air, is as 1103 to 1000 nearly.—Eſſay on Phlogiſton, p. 13.

rendered lighter than common air, the ſurrounding atmoſphere preſſes upon it on all ſides, and prevents it from deſcending in the glaſs.

Mayow, then, here met with what we now term azot, and diſtinguiſhed it by the only two properties by which we now diſtiſtinguiſh it, *viz.* its being unfit for reſpiration and combuſtion. He was not aware, that in reſpiration and combuſtion, the carbonic acid gas is given out. The chemical reader well knows, that this gas is heavier than common air*; but that a conſiderable quantity of caloric being diſengaged, both by the animals and burning bodies, the carbonic acid is rendered lighter, and conſequently aſcends. Mayow did a great deal, in thus decompoſing atmoſpheric air, and diſtinguiſhing between the fire-air particles and azot; it was enough for him to have ſhewn, that only one part of the air ſupported life and combuſtion. He admires the wiſdom of Providence, in thus carrying off from the earth, air unfit for the purpoſes of life. How much more would he have praiſed the benevolence of the Deity, had he known, that carbonic acid is of greater ſpecific gravity than atmoſpheric air; but to remedy the evil that would ariſe to animal life, from its accumulation, it is made the food of plants, and is readily abſorbed by water, to which, no doubt, the agitation of tempeſtuous weather greatly contributes. Mayow maintained, that the elaſticity of the air depended, in a great meaſure, on the fire-air particles. He entertained, however, much doubt upon this ſubject; and did not deny, that air, deprived of fire-air particles, was alſo elaſtic. There is ſomething elſe then neceſſary to produce this effect. It is, therefore, very probable, he ſays, that a ſubtile active matter, being interſperſed between the particles of the air, and conſtantly keeping them in motion, not a little conduces to the expanſive power of air; and by this means, it would appear, that the elaſtic force of the air is cauſed from the application of heat. The following experiment was made to determine, how this elaſticity was diminiſhed.

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* The weight of fixed air, procured from calcareous ſpar, by muriatic acid, is to that of common air, as 1500 to 1000; and that of azot, is as 985 to 1000.—Kirwan, p. 15—17.

(N) Let a ſtick, equal in length to the wideſt part of a glaſs cucurbit, be placed within it, ſo that its extremities may reſt upon the ſides, as is ſeen in *Fig.* 4. From this tranſverſe ſtick, ſuſpend a glazed earthen-ware veſſel, capable of containing about four ounces, half full of nitrous acid. Throw acroſs the ſtick, directly over the veſſel containing the acid, pieces of iron, tied to a cord, the other end of which muſt be ſufficiently long to come out from under the cucurbit. The orifice of the inverted cucurbit is to be immerſed about five fingers breadth below the water. After the water is raiſed in the cucurbit, by the ſyphon, ſome is to be taken from the outer veſſel, until that within ſtand about three fingers breadth higher. Let every thing remain thus, till the included air, heated by the hands, return to its former ſtate; the height is then to be marked with ſtrips of paper. The pieces of iron are then let down into the acid, by means of the cord. A violent ebullition with heat will immediately take place, and the water will be depreſſed by the diſengaged gas. In about twenty minutes, or rather when the water has been depreſſed about three fingers breadth by the gas, the pieces of iron are to be raiſed from the acid: you will then ſoon ſee the water riſing gradually in the glaſs; and in an hour or ſo, it will be elevated far above the height marked by the ſtrips of paper, ſo that one-fourth of the ſpace, hitherto occupied by the air, will now be filled by the water; and indeed, in no long time, the water thus raiſed, will deſcend to its former level ' I ſuſpect ſome miſtake here, ſays Dr. Beddoes; the author ſays nothing more of this ſtrange new depreſſion of the water; how can any air be generated in theſe circumſtances? Did one of the pieces of iron, at any time come off the bunch, and remain behind in the acid? I wiſh the paſſage were out of the book; or at leaſt, that any one would explain it to me.' It does not appear difficult of explanation; it is well known, that oxygen unites with azot in various proportions, forming the nitrous oxyds. During the ſolution of metals in the nitrous acid, as the oxygen is robbed from the acid, a gas is generated, which is ſometimes azot or nitrous air, with different proportions of oxygen; for it is not the ſame quantity of oxygen which unites with azot to form nitrous air; and in proportion to the quantity of oxygen, ſo is its abſorption by the water more or leſs rapid. In this way, we may eaſily account for the new depreſſion of the water. After the pieces of iron were withdrawn, ſome portion of the acid would remain adhering to them, by which azot, or nitrous gas, with a ſmall proportion of oxygen, would be diſengaged; theſe were not abſorbed by the water, and the air could not be again diminiſhed in bulk, becauſe the oxygen had already been exhauſted by the nitrous gas firſt diſengaged, and we know too, that nitrous gas is not rapidly abſorbed by water; hence it muſt have ſuffered a new depreſſion from the diſengaged gas. It is evident, that in this experiment, Mayow procured nitrous air; and it is rather curious, he does not mention the turbid fumes which it produces in its union with fire-air particles. This appearance, as far as we know, was firſt mentioned by Dr. Hales, who procured nitrous air from Walton pyrites and ſteel filings, with the nitrous acid.*

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* Vide Statical Eſſays, Vol. II. p. 280.

If the pieces of iron be let down a ſecond and a third time into the acid, freſh gas will be diſengaged, but the water will not riſe, as in the firſt caſe. His method of explaining the riſe of the water deſerves attention, notwithſtanding that it is a little confuſed, from the want of diſtinct ideas, with reſpect to the tranſition of bodies, from the aeriform to the ſolid ſtate. (O) The effervеſcing particles, he obſerves, produced in the firſt ebullition, being mixed with the particles of the air, deſtroy them; hence the water not only roſe into the ſpace left by the condenſed gaſes, but alſo into that which the deſtroyed air formerly occupied: but when the air in the glaſs was in a great part deſtroyed by the firſt fermentation, or rather had its elaſticity as much as poſſible diminiſhed, it happened, that after the gas was diſengaged a ſecond time, the water would not riſe into the glaſs, as in the firſt inſtance, except as far as the generated gas was condenſed. We may here perceive, that Mayow makes a diſtinction between the diminution of the air's elaſticity by the gas, and the condenſation of the gas itſelf. In the firſt inſtance, therefore, the water would riſe from two cauſes, *viz.* the loſs of elaſticity in the air, and the condenſation of the gas; but in the ſecond caſe, when the elaſticity had been already deſtroyed by the gas previouſly generated, the riſe of the water could be only attributed to the condenſation of the nitrous air. Such is the manner in which Mayow accounted for this extraordinary phænomenon, which paſt unnoticed, and neglected for more than half a century,* when it was obſerved by Dr. Hales, who although he quotes Mayow on reſpiration, took no notice of this curious circumſtance. In the diſengagement of airs, from Mayow's effervefcing mixtures, he obſerved, that the water ſometimes did not riſe again, after its depreſſion. He accounts for this in the following manner: (P) When the diſengaged gaſes occupy more ſpace than is relinquiſhed by the loſt air, then, although the elaſtic power of the air be diminiſhed, that circumſtance will not take place: hence when gaſes are procured from the nitrous acid and a fixed ſalt (carbonie acid gas)

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* The Vice-Chancellor's Imprimatur for Mayow's Tracts, is dated 1673, Sir I. Newton's, as P. R. S. for Hales's 1ſt Vol. in 1726—27.

gas) and from ſulphuric acid and iron (hydrogen gas), in the manner already deſcribed, the water will not riſe above its priſtine height. When Mayow found, that part of the air could thus be condenſed, and as it were loſt, he ſet on foot an enquiry, to diſcover whether air could be generated anew. During this inveſtigation, he collected various gaſes, with which he made ſeveral trials, with reſpect to their fitneſs for reſpiration and combuſtion. In the following experiment, the reader will perceive, that he procured nitrous air in a ſeparate ſtate.

(Q) Place equal parts of nitrous acid and water in a large glaſs, then plunge into it a ſmall glaſs goblet, till it be quite full, and after having introduced into its mouth two or three iron globules, invert it on the bottom of the other veſſel, as is ſeen in *Fig.* 3, taking care to prevent the globules from falling out, by applying the finger to the orifice. In a ſhort time, the acid will corrode the iron, and raiſe an efferveſcence; vapours, in the form of bubbles, will aſcend to the top of the glaſs, and there conſtitute a gas, by which the water will be gradually depreſſed. After the glaſs has been filled with the gas, the iron is to be withdrawn, taking care that the orifice of the glaſs does not riſe above the ſurface of the fluid. We ſhall ſoon ſee the gas becoming gradually condenſed, and the liquor riſing into its place. About one-fourth of the gas will remain in the glaſs, which cannot be condenſed by the ſevereſt cold. Some condenſation would, no doubt, take place, after the heat was diſſipated, which was generated during the effervefſcence; but the diſappearance of the nitrous air, is to be wholly attributed to its abſorption by the nitrous acid and water.

(R) If in the place of the nitrous, we uſe the vitriolic acid, mixed with water, or if the effervefſcence produced, as in the laſt experiment, be very ſlow, or if the globules of iron be allowed to remain a day or two in the glaſs, the gas generated in this way will ſuffer no condenſation. The reader will perceive, that hydrogen was procured in the firſt inſtance, which we know is not abſorbed; and in the two laſt caſes, the liquor, on account of the nitrous air paſſing ſlowly through it, would become ſaturated, and no diminution would take place in that which roſe to the top of the glaſs: ſome azot, too, would undoubtedly be diſengaged. It is difficult to ſay, obſerves Mayow, whether this gas be really air or not; but it is evident, from the following experiment, that it is equally capable of expanſion.

(S) Take a glaſs tube, of the ſize of a gooſe quill, and about four fingers breadth long, hermetically ſealed at one end; let fall into it a drop of water, and note down the ſpace occupied by the drop, upon paper paſted on the outſide; then let fall ſome more drops, noting down the height; and ſo on. The open extremity of the tube is then to be cloſely cemented, into the orifice of another glaſs, perforated at both ends, as is ſeen in *Fig.* 5. The tube and glaſs veſſel are then to be filled with water, and inverted in another, containing the ſame fluid. The gas is then to be transferred into this apparatus, in the following manner: (T) Let a ſmall veſſel, filled with water, be introduced under the orifice of the other, containing the gas, and be thus removed to the apparatus, containing the veſſel and tube; then bring the mouth of the glaſs, filled with the gas, to the orifice of the one filled with water, and incline the former in ſuch a manner, that the gas may paſs into the latter, which may be thus filled with gas, although a ſmall quantity will ſuffice for our purpoſe. The veſſel and tube containing the gas, are then to be placed in Boyle's air-pump, by introducing under it a veſſel, ſufficiently large to receive its orifice. (V) When the apparatus was thus adjuſted, he found, by exhauſting the air-pump, that the gas expanded, ſo as to fill the glaſs veſſel; and by comparing the capacity of the whole veſſel, with the ſpace generaly occupied by the gas, and marked on the paper, he found, from frequent repetition of the experiment, that the gas expanded about two hundred times its original bulk; and had the preſſure of the water been removed, he ſays, it would have made double that expanſion, and common air managed in the ſame way, does not expand more. (W) This experiment, then, decidedly proved the gas to have one property, common with atmoſpheric air. But this does not argue its reſpirability; for air in which an animal has died, or candle been extinguiſhed, is capable of an expanſion, equal to that of any other air; and we know it to be deprived of the nitro-aerial and vital particles. Although therefore, theſe gaſes will expand themſelves, when atmoſpheric preſſure is removed, we muſt not imagine, that they are reſpirable. This analogical reaſoning did not ſatisfy Mayow, he was determined to reſt his opinion upon matter of fact; to clear up therefore all doubt and ſuſpicion, on the ſubject, he made the following experiment.

(x) Let a mouſe, confined in a trap, be placed upon a ſupport, under an inverted glaſs veſſel, as is ſeen in *Fig.* 6. Let all things remain in this ſituation, till the animal dies: the time it lived is to be exactly noted. The mouſe is now to be withdrawn, and another placed in the ſame ſituation, and under ſimilar circumſtances, taking great care, that the ſame quantity of air be uſed in this, as in the firſt caſe. The apparatus being thus adjuſted, ſome gas is then to be thrown up into the veſſel, about twice or thrice the quantity it contains of air. In this caſe it will be found, that the animal will live but very little longer than in the firſt, when the gas was not confined with it. The reaſoning he applies, ſhews how accurately he attended to the reſult of his experiments. If the gas were really air, and fit to ſupport life, the ſecond animal employed ſhould have lived twice as long as the firſt. An obſervation which he adds is extremely nice; the reaſon why the animal lived ſomewhat longer in the ſecond, than in the firſt inſtance is, becauſe the air being diluted with gas, the fire-air particles are more gradually conſumed.

Mayow does not mention, from what he procured the gas made uſe of in this experiment; but from the reſult we muſt conclude, that it was the hydrogen, otherwiſe the mouſe would have been inſtantly killed by the nitrous gas. From a review of theſe experiments, we cannot but admire the neatneſs and accuracy with which our ingenious author contrived and executed his plans. The method he uſed for transferring the gaſes from one veſſel to another claims our admiration, and muſt have been the reſult of much painful labour and attention. We have ſeen how completely he has anticipated the modern chemiſt, in this mode of experiment. One method, however, which he uſed, is ſufficiently curious, and certainly deſerves notice. He wiſhed to transfer, from one veſſel to another, the air which had been de-oxygenated by reſpiration and combuſtion. To effect this, he had recourſe to the following expedient. A phial is filled with water; round its neck is tied a piece of ſtring; another piece, of ſufficient length to ſuffer both its extremities to come out from under the edge of the glaſs, is thrown acroſs the tranſverſe ſtick in the apparatus, *Fig.* 4; one end of this is tied to the bottom of the phial; by drawing down, therefore, the other end of the ſtring, the phial will be raiſed above the level of the water in the inverted glaſs, and will conſequently be filled with air; again, by drawing down the ſtring to the neck of the phial, it can be withdrawn, and transferred where we pleaſe. He did this with a view of diſcovering, by his meaſuring apparatus, *Fig.* 5, whether the air, in which an animal had died, or candle been extinguiſhed, was capable of expanſion, which he found was the caſe. We may obſerve, that our ingenious author did not diſcriminate between the different gaſes he obtained. One diſtinction, however, he made, but it did not occupy his attention, *viz.* that ſome of his gaſes underwent, what he called, condenſation; but which was their abſorption by the liquors, over which they were procured, while others did not. He ſeems to have conſidered them all the ſame, in their properties; and only knew they were unfit to ſupport animal life and combuſtion. Let it be remembered, that after the lapſe of more than a century, it required the innumerable experiments, and diligent inveſtigation of Prieſtley and Cavendiſh, to diſtinguiſh rightly between the gaſes; and even after this period, the effects of noxious airs upon animal life were not known, the experimentaliſt conſidering the qualities of the unreſpirable airs as ſimilar. This led to the different concluſions which Mr. Kite and Mr. Coleman drew from their experiments*; but the cauſes of the error we have pointed out, in our paper to the Royal Society, on the effects of the different gaſes on the influence, termed by Galvani, animal electricity.

(A) Si cucurbitula, flammâ repleta, cuti applicetur, flammam mox extinctum iri, ſpatiumque

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* Diſſertation on Suſpended Reſpiration, by E. Coleman, p. 24.

que intra cucurbitulam incluſum, pene vacuum fore. P. 97.

(B) Probabile eſt, aerem flammæ confertim immiſceri, utpote cui in pabulum cedit; ita ut ne minima quidem flammæ pars ſit, in quâ aeris aliquantulum non exiſtit.

(C) Nempe candela accenſa, ita in aqua collocetur, ut ellychnium accenſum digitos circiter ſex tranſverſos aquæ ſuperemineat: dein cucurbita vitrea ſatis alta, inverſaque, lucernæ iſte ſuperimponatur, prout in *Fig.* 1, oſtenditur. Curandum eſt autem hic, ut ſuperficies aquæ intra vitrum incluſæ, altitudinem aquæ exterioris æquet; quo autem id fiat, ſyphonis incurvati ejus alterum intra cucurbitæ cavitatem, antequam eadem in aquam demittitur, includatur crure altero exterà eminente. Syphonis iſtius uſus eſt, ut aer in alembico incluſus, et ab aquâ ei ſubſtratâ, dum vitrum in aquam demittitur, compreſſus, per ſyphonis cavitatem exeat. Cum autem aer per ſyphonem iſtum exire deſinit, ſyphon ſtatem eximatur, ne aer poſtea per eundem in vitrum irruat. Brevi aquam in cucurbitæ cavitatem, cum adhuc lucerna deflagrat, gradatim aſſurgentem percipies. P. 99.

(D) Nempe materia quævis combuſtibilis, quæ facile flammam concipiet, in cucurbitâ quam capaciſſimâ, inversa, ſuſpendatur, prout in *Fig.* 1, oſtenditur (ipſe fruſtulum camphoræ, cui lentei ad nigredinem, uti moris eſt, calcinati, ſulphureque liquefacto intincti, tantillum affigitur, ſuſpendere ſoleo). Quo facto, cucurbita ea inverſa, ad digitos circiter 10 aquæ, ita immergatur, ut aqua intra vitrum incluſa, aquæ exterioris altitudinem æquet, quod ope ſyphonis fieri poteſt; ac demum aqua exterior eouſque exhauriatur, donec aquæ interioris, altitudo exteriori ſupereminet, quo melius in conſpectum veniat; tum altitudo aquæ interioris, chartulis hic illic lateribus vitri affixis, notetur. Jam vero cucurbita iſta radiis ſolaribus exponatur, et camphora in eadem incluſa, ope vitri uſtorii accendatur; quo facto, aquam interiorem, ob particularum ignearum exagitationem aeriſque rarefactionem, intus deſcendere percipies. Poſtquam lucerna expiravit, cucurbita una cum vaſculo, cui incumbit, a radiis ſolaribus amoveatur, quo ſe aer in ea incluſus denuo refrigeſcat, et ad priſtinum ſtatum redeat, atque ita aquam interiorem, ſupra metam primo notatam, elevatam eſſe invenies. P. 100—101.

(E) Nempe veſica madefacta, orificio circulari vaſis cujuſvis ſuperextendatur, eidemque alligetur; dein cucurbitula, in quâ animalculum, puta, mus incluſus eſt, veſicæ prædictæ ſtrictim applicetur, ut in *Fig.* 2. delineatur. Cucurbitulam, poſt breve temporis ſpatium, veſicæ iſti firmiter affixam, et inſuper veſicam, qua eadem cucurbitulæ ſubſtrata eſt, ſurſum in vitri cavitatem impulſam videre eſt, haud ſecus, ac ſi cucurbitula ea, cum flammâ ei incluſa, applicata fuiſſet; atque hoc, animale ad huc ſpirante, continget. Et quidem, animalculum cucurbitulæ cuti applicandæ impoſitum, flammæ vicem aliquantulum ſupplere poteſt. Ex quibus liquet, vim elaſticam aeris, per animalis reſpirationem, imminutam eſſe. P. 103-4.

(F) Nempe animal, in carcere idoneo incluſum, in cucurbita vitreâ ſuſpendatur, prout vaſculum in *Fig.* 4. Dein vitrum inverſum aquæ aliquantulum, ita immergatur, ut aqua intra vitrum incluſa, aquæ exterioris altitudinem æquet, quod ope ſyphonis incurvati fieri poteſt. Aqua exterior aliquantulum exhaureatur, quo altitudo aquæ interioris melius conſpici poſſit, quæ chartulis hic illic vitri lateribus affixis, notetur. Brevi, aquam in vitri cavitatem ſenſim aſſurgentem videbis, licet calor ab animalis præſentiâ in vitro iſto excitatus, item halitus ab eodem exeuntes, potius contrarium efficere viderentur; et quidem experimento, cum animalibus variis facto, compertum habeo, aerem in ſpatium ex parte circiter decimâ quartâ minus, quam antea, per animalium reſpirationem redactum eſſe. P. 104-5.

(G) Ex dictis, certo conſtat, animalia reſpirando particulas quaſdam vitales eaſque elaſticas ab aere exhaurire; ut minime jam dubitantum ſit, aereum aliquid, ad vitam prorſus neceſſarium, ſanguinem animalium reſpirationis ope ingredi. Ex quibus manifeſtum eſt, aerem per animalium reſpirationem, haud multo ſecus, ac per flammæ deflagrationem, vi ſuâ elaſticâ deprivari; et utique credendum eſt, animalia ignemque particulas ejuſdem generis ex aere exhaurire, id quod ſequenti experimento, magis adhuc confirmatur. P. 106-7-8.

(H) Nempe animalculum quodvis, unà cum lucernâ, in vitro includatur. Brevi lucernam iſtam expirantem videbimus, neque animalculum diu tedæ ferali ſuperſtes erit. Etenim obſervatione com-

pertum

pertum habeo, animal una cum lucernâ, in vitro incluſum, haud multo plus, quam dimidium temporis iſtius, quo aliàs viveret, ſpiraturum eſſe. P. 108.

(I) Porro conatus ſum materiam combuſtibilem, in vitro, una cum animale ſuſpenſam, poſtquam animal in eodem ſuffocatum eſt, ope vitri uſtorii accendere; verum experimentum non ſucceſſit. Veriſimile eſt autem, aerem, qui vitæ ſuſtinendæ inidoneus eſt, etiam ad flammam conflandam ineptum eſſe. P. 110.

(K) Si animalculum, veluti mus aut avis, in ſummitate vitri includatur, idem multo citius morietur, et aqua ſubſtrata multo minus elevabitur, quam ſi animalculum idem, in inferiori parte vitri collocatum eſſet. Hoc autem maxime manifeſtum erit, ſi aves, aut mures duo, ſimul, unus in ſuperiori, alter in inferiori parte vitri incluſi fuerint; ita enim animalculum, in inferiori vitri parte poſitum, alteri aliquandiu ſuperſtes erit. P. 124—5.

(L) Verum arbitrari fas ſit, particulas aeris, è pulmonibus animalis egeſtas, ob particulas nitro-æreas partim ex ipſis exhauſtas, leviores fieri, eoque ad ſummitatem vitri aſcendere. P. 125.

(M) Quinimo ſi cucurbita vitrea inverſa, in aere ſuſpendatur, et dein lucerna in eandem immittatur, lucernam brevi expirantem percipies; quippe aer, in vitro iſto contentus, ob lucernæ deflagrationem, ad ignem ſuſtinendum ineptus redditur, cum vero idem reliquo aere levior ſit; hinc fit, quod aer ambiens eum ſurſum impellit, neque facile è vitro deſcendere permittet. P. 126.

(N) Nempe bacillus diametro cucurbitæ vitreæ, qua eadem maxime patet, æqualis, in cucurbitam imponatur, dein idem tranſverſim collocatus, deorſum trahatur, quouſque bacilli extremitates utræque vitri lateribus innixæ, ab iiſdem ſubfulciantur, prout in *Fig.* 4. cernitur. Vaſculum fictile intus vitrefactum, unciarum liquoris circiter quatuor capax, ope unci ferrei ei adaptati, de bacillo iſto tranſverſo ſuſpendatur, idemque ad dimidium plus minuſve ſpiritu nitri impleatur. Porro fruſtula aliquot ferri in faſciculum colligata, ope funiculi de bacillo prædicto ſuſpenſi, e directo ſupra vaſculum illud ſuſpendatur, funiculus autem iſte tantæ longitudinis eſſe debet, ut ejus extremitas altera ad vitri orificium protenſa, foras propendeat. Orificium cucurbitæ iſtius inverſæ ad digitos circiter quinque aquæ immergatur ita—

aqua exterior eouſque exhauriatur, donec aqua interior digitos ciciter tres eidem ſuperemineat; atque ita maneant omnia, donec aer in vitro incluſus manibus tractantis calefactus, ad priſtinum ſtatum redeat: tum altitudo aquæ interioris chartulis hic illic vitri lateribus exterius affixis, notetur. Jam vero fruſtula prædicta, ope funiculi, in vaſculum, in quo ſpiritus nitri collocatus eſt, demittatur; atque ita brevi æſtus admodum intenſus excitabitur et aqua interior ab halitibus inde ortis ſtatim deprimetur. Poſtquam æſtus iſtius modi per tertiam plus minuſve horæ partem duraverit; ſeu potiùs cum aqua interior ad digitos circiter tres ab halitibus excitatis depreſſa fuerit, fruſtula iſta ferri ope funiculi e vaſculo eleventur. Poſt brevi temporis ſpatium aquam interiorem gradatim aſſurgentem et intra horam unam aut alteram longè ſupra altitudinem ab initio notatam, elevatam eſſe percipies; ita ut pars circiter quarta ſpatii, quod in vitro eo antea ab aere occupatum eſt, nunc ab aqua intus aſſurgente teneatur. Et quidem aqua eo modo in vitrum elevata, neque poſt longum tempus, ad priſtinam metam deſcendet. P. 136-7-8.

(o) Par-

(O) Particulæ efferveſcentes ab æſtu priori exeuntes, particulis aereis confertim immixtæ, eas deperdiderunt; unde factum eſt, ut aqua non tantum in ſpatium ab halitibus condenſatis, ſed etiam ab aere abſumpto relictum, elevata ſit: cum vero aer in vitro eo ex maximâ parte per primam fermentationem deperditus eſt, ſeu potius quoad vim ſuam elaſticam, quantum fieri poterat, imminutus eſt, hinc fit, ut poſt æſtum ſecundâ vice factum, aqua non niſi, quatenus halitus excitati condenſationem patiebantur, et proinde multo minus, quam in priori caſu in vitrum aſcenderet. P. 141.

(P) Quandocunque halitus generati plus ſpatii occupabunt, quam ab aere deperdito relinquitur, tunc, utcunque vis elaſtica aeris imminuitur, illud tamen in vitro prædicto non comparebit. Atque hinc eſt, quod ſi æſtus a ſpiritu nitri et ſale fixo commixtis, uti etiam ab oleo vitrioli et ferro, modo prædicto, in vitro excitatus fuerit, aqua tamen, ſupra altitudinem priſtinam non aſſurget. P. 145.

(Q) Nempe ſpiritus nitri et aqua fontana, in æquali quantitate commixta, vitro ſatis amplo imponantur; dein vitrum parvulum mixturæ iſti ita ſubmergatur, ut idem liquore eo penitus repleatur, quo facto, globuli duo vel tres, e ferro compoſiti, orificio vitri hujus indantur, idemque inverſum fundo vitri alterius incumbat, prout in *Fig.* 3, delineatur, cavendo tamen, ne globuli iſti è vitro excidant; quo autem hoc præcaveatur, orificium vitri iſtius digito obturetur, donec idem fundo vitri alterius innititur. Poſt brevi temporis ſpatium, menſtruum illud acidum globulos ferreos corrodet, et cum iiſdem inſigniter efferveſcet; halituſque ſub bullularum formâ, ab æſtu eo excitati, ad ſupremam vitri partem aſcendent, auramque ibidem component, quæ ſenſim aucta aquam ſubſtratam gradatim deprimet. Poſtquam vitrum iſtoc aura iſtius modi penitus impletum eſt, idem parum elevetur, quo globuli ferrei ex eodem elabantur, qui è liquore eximendi ſunt; cavendo tamen, ne vitri orificium ſupra liquorem attollatur. Atque ita videbimus auram iſtam gradatim condenſari, liquoremque ſubſtratum in locum ejus aſcendere; etenim vitrum ex parte circiter quartâ uſque implebitur, auraque ea, tempeſtate frigidiſſima exiſtente, nunquam tamen in liquorem condenſabitur. P. 161-2-3.

(R) Si loco ſpiritus nitri, oleum vitrioli cum aqua commixtum ſubſtituatur; aut ſi fermentatio valde remiſſa, modo prædicto, excitetur; aut etiam ſi globuli ferrei, in vitro eo per diem unum aut alterum manere ſinantur; tunc aura, eo modo generata, vix omnino condenſationem patietur. 163.

(S) Nempe vitrum tenue, ad magnitudinem calami anſerini, quatuor plus minus digitos longum, in alterâ extremitate hermeticè ſigilletur; dein gutta una aquæ, orificio ejus alteri aperto, inſtilletur, et in chartulâ vitro exterius affixâ deſignetur, quantum vitri iſtius ſpatium gutta ea occupat; tunc gutta inſuper altera, et ita deinceps aliæ vitro ei, ut prius immittantur, ſpatiumque ab iis impletum, in chartulâ prædictâ notetur. Vitri iſtius extremitas aperta, orificio anguſtiori vitri alterius, in utrâque extremitate perforati, indatur, et dein foramen illud ope cementi idonei ſtrictim obturetur, prout in *Fig.* 5. delineatur. Vitrum hoc, eo modo paratum, aquæ in vaſe idoneo contentæ, ita ſubmergatur, ut orificio ejus ſurſum verſus ſpectante, aer ex eodem totus exeat, et aqua ejus locum ſubeat, curando diligenter, ut etiam vitrum aquâ repleatur; dein vitrum,

trum, iſtoc aquâ impletum, invertatur, idemque fundo vaſis alterius incumbat, atque ita ſingula maneant. P. 164-5.

(T) Nempe vaſculum parvulum, orificii tamen vitri, in quo aura exiſtit, capax, eidem tamen ſupponatur; et dein vaſculum illud liquore prædicto repletum, una cum vitro inverſo, in quo aura continetur, ei incumbente, in vaſculum illud transferatur, in quo vitrum primo deſcriptum collocatum eſt, atque orificium vitri, in quo aura exiſtit, orificio vitri alterius aquâ repleti ſupponatur, vitrumque illud eouſque inclinetur, quoad aura in eo contenta prorumpit, et in vitrum alterum aſcendit, quod hoc modo aurâ iſtâ impleri poteſt; quanquam ſatis eſt, ut aeris aliquantulum eidem immittatur. P. 165.

(V) Poſquam aer ex parte exhauſtus eſt, aura in vitro prædicto incluſa, ultra vitri cavitatem ſe expandet. Itaque ſi ſpatium vitri totius, ope aquæ ei guttatim inſtillatæ, commenſuretur, idemque cum ſpatio eo vitri tenuioris, quod aura reliqua adimpleverat, conferatur, compertum erit, in quantum aura prædicta ſe expanderat. Atque experimento *ſæpe* repetito, compertum habeo, auram iſtius modi, plus quam ducenties ſe expandiſſe; et quidem ſi eadem ab aquæ ambientis preſſurâ liberata fuiſſet, in ſpatium circiter duplo majus ſe expandiſſet, neque quidem aer vulgaris, eodem modo diſpoſitus, in ſpatium majus ſe explicabit. P. 166-7.

(w) Quanquam aura ab æſtu prædicto generata, vi elaſtica non minori, quam aer vulgaris imbuta ſit, tamen non exinde ſequitur, eam reverâ aerem eſſe talem, *viz.* qui particulis vitalibus, igneiſque donatur; quippe aura iſta, in quâ animal aut lucerna expirarunt, vi elaſticâ æque ac aer inviolatus pollet; et tamen eadem particulis nitro-aereis, vitalibuſque, deſtituitur. P. 169.

(x) Animalculum puta mus in carcere anguſto collocatus, in ſummâ vitri inverſi cavitate collocetur, ſuſtentaculo idoneo ei ſuppoſito, ut in *Fig.* 6, delineatur. Ita maneant omnia, uſque dum animal moriatur; ſpatiumque temporis quo animal in vitro eo ſpiravit, diligenter obſervetur. Jam vero animal mortuum eximatur, atque animal aliud vivum, in ejus loco ſubſtituatur, et veluti prius, intra vitrum aquæ immerſum includatur, curando diligenter, ut aeris eadem quantitas, quæ prius, intra vitrum includatur: quo facto, aura prædicta in vitrum, in quo animal collocatum eſt, tantâ copiâ modo antea oſtenſo transferatur, ut aura ita immiſſa, bis aut etiam ter aerem vulgarem intra vitrum iſtoc ab initio incluſum ſuperet. Comperiemus animalculum, in vitro eo, haud multo diutius ſecundâ vice vitam degiſſe, quam alterum primâ, cum, *viz.* aura prædicta vitro iſti non imponebatur. Sin autem aura iſta revera aer, eademque vitæ ſuſtinendæ idonea eſſet, animalculum ſecundâ vice immiſſum, duplo longius quam præcedens perduraſſet. Quod vero animalculum, aurâ prædicta, vitro in quo idem extitit, impoſitâ, paulo diutius quam alias vixerat, ratio eſſe videtur; quoniam aer, intra vitrum iſtoc incluſus, ob auram ei admixtam magis gradatim, neque adeo confertim ac aliàs ab animalculo hauriri et violari, poterat. P. 169-70-1.

SECT. VI.

OF RESPIRATION.

There is no part of the animal œconomy, which would ſooner have excited the attention of the obſerver, than the neceſſity of continued reſpiration to the life of animals. The moſt common daily occurrences muſt have pointed out, that we could diſpenſe with any thing but air for a conſiderable time. (A) So great a demand, ſays the venerable Hippocrates, have our bodies for air, that we may refrain, with impunity, from all other kinds of food, for two, three, or more days; but if any thing ſhould interrupt the communication of the air with our bodies, for a ſhort time only, we muſt unavoidably periſh, ſo neceſſary is the preſence of air to our being. A crowded meeting, in places of public amuſement, or courts of juſtice, muſt have preſt upon the obſervation of the earlieſt philoſophers, the neceſſity of the preſence of freſh air. The decompoſition which daily takes place in different ſubſtances, from fermentation, preſented to their examination various exhalations; but theſe were all confounded under the name of air, or vapour. They beheld the beauteous ſtructure of the animal frame governed by certain laws, which did not exiſt one moment without the preſence of air; hence it was reaſonable to infer a connection between the functions of the ſyſtem and that fluid. A diſtinction was alſo made between the air neceſſary, and the external atmoſphere; but the manner of its action, and the nature of its effects, were entirely unknown. We are ſurpriſed that they knew ſo much, when we conſider how little they were acquainted with the operations of the animal œconomy; and for the greateſt part of their knowledge on this ſubject, they were indebted to the labours of one man, to the acute obſervation and regular attention of the venerable Father of Phyſic. A ſpirit, it was ſaid, pervaded all nature, gave laws to the animal œconomy, and regulated the motion of the heavens, and this ſpirit was derived from the air, but was different from it. Hippocrates mentions, that all animated nature is ſupported by a triple nouriſhment, by meat, drink, and a ſpirit, *σιτᾶ, ποτα, και πνευματα*. Since exiſtence was inſeparable from this ſpirit, it was conſidered as the ſoul and great mover of all the operations of the œconomy, and became ſynonymous with life.[*]

Dum memor ipſe mei, dum ſpiritus hos regit artus.[†]

Afterwards, when the nerves of the body were found to have ſo great a ſhare in its motions, this ſpirit was ſuppoſed to reſide chiefly in them; hence the terms nervous, vital, and animal ſpirits. The word ſpirit was uſed to convey the idea of an immaterial quality, which might exiſt *per ſe*, but without which all the animated creation would be devoid of life and vigour. After the time of

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[†] The lungs are ſo uſeful to us, as to life and ſenſe, that the vulgar think, our breath is our very life, and that we breathe out our ſouls when we die. Suitably to this notion, both *anima* and *ſpiritus* in Latin, and *πνευμα* in Greek, are derived from words that ſignify both breath and wind, and *efflare*, or *exhalare animam*, ſignifies to die.—Ray, p. 13, Part II.

[†] Æneid Lib. VI. V. 336.

Hippocrates, it was the faſhionable philoſophy, that heat or fire was that quality, under the name of ſpirit, which produced harmony and order in the revolutions of the animated world. ‘Hic noſter ignis, ſays Cicero, quem uſus vitæ requirit, confector eſt et conſumptor omnium, idemque quocumque invaſit, cuncta diſturbat ac diſſipat. Contra ille corporeus, vitalis eſt et ſalutaris, omnia conſervat, alit, auget, ſuſtinct, ſenſuque adficit’.* In the abſence of heat, it was obſerved, that the creation became torpid and cramped in her operations, ‘all nature ſickened,’ and the hybernating animals retired to their caves and ſecret places, to remain until this vivifying principle reſtored animation to the face of the world; hence probably, aroſe the idea of fire or heat, being the ſoul and life of created being; and hence we may derive the terms vital flame, igneous ſpirit, &c. It was the breath of liſe, and impreſt upon the world, *ab origine*, by the Deity.

> Igneus eſt ollis vigor, et cœleſtis origo
> Seminibus†.

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* De Natura Deorum, p. 144.

† Æneid. Lib. VI. v. 730.

In the time of Hippocrates then, the ſpirit which animated the human frame, was derived from the air, and afterwards the diſciples of the Platonic ſchool conſidered it as the ſame with fire. They made no diſtinction, however, between life itſelf and its ſtimulus. The body was the machine in which this ſpirit performed its operations; but they did not imagine that, connected with the body, was a principle, independent of this ſpirit, but to which it acted as a ſtimulus. This beautiful illuſtration of the œconomy was reſerved for a diſtant period. No wonder then, that the antients were perplexed in their reaſonings on this ſubject, ſince it required ſo many ages to diſcover a principle, without the knowledge of which, we ſhould have ſtill remained ignorant ſpectators of the phænomena which daily occur in the animated frame. It is a curious co-incidence in the hiſtory of ſcience, that the two qualities which the antient philoſophers conſidered as neceſſary to life, and which they thought were different, ſhould be found combined in a ſingle ſubſtance by the diſcoverers of modern times. We cannot be ſurpriſed to find, that the two moſt powerful agents in nature, air and fire, ſhould have been called forth to explain the phænomena, which occurred to the obſervation of the philoſopher. General obſervations, however, were only made without being particulariſed by any determinate ideas on the ſubject. This could not be otherwiſe, when we conſider how little men were in the habit of making experiments, and how much it was neceſſary that the arts ſhould be improved, before it was poſſible that experimental knowledge could be acquired. So near is the connection between the arts and ſciences, and ſo much does the practice of the philoſopher depend upon the improvement of the workman's art. Ridiculous hypotheſis prevailed, in proportion as the theoriſt was perplexed in his ideas, until the great Lord Verulam arreſted the progreſs of theoretical reaſonings, not founded on direct experiment. Soon after this Mayow appeared, and in 1668, publiſhed his Treatiſe on Reſpiration. The doctrine laid down for this important function, evidently ſhews, the great attention of an ingenious mind, eagerly applied to the object of its purſuit. Unſatisfied with having diſcovered that only one part of the atmoſphere ſupports vitality, he extends his enquiry, to learn what *this* performs in the ſyſtem; and we ſhall ſee how accurately he has ſtruck out the truth. He rejects the prevailing theory of his time, that reſpiration is to cool the blood; on the contrary, he maintains that it produces the oppoſite effect. He denies that it is intended to tranſmit the blood from the right to the left ſide of the heart. (B) Indeed, it is evident, that the blood can paſs through the lungs without their motion; for if blood, or any other fluid, be injected by the means of a ſyringe into the pulmonary artery of a dead animal, it will very readily paſs into the left ventricle of the heart. Any one may experience this in himſelf; for although reſpiration be ſuppreſſed for a time, yet the pulſation of the arteries is ſufficiently firm in the wriſts, which certainly could not happen, unleſs the blood at that time paſſed through the lungs, to the left ventricle of the heart. He does not however deny, that the action of the thorax, aſſiſts the tranſmiſſion of the blood; neither will he allow, that reſpiration is to break down the denſe venous blood, becauſe impure air very well anſwers the purpoſe of giving motion to the lungs, and agitation to the blood, but it does not ſupport life. Having thus refuſed his aſſent to the commonly received doctrine of reſpiration, it was incumbent on him to deliver his ideas on the ſubject, and to urge his objections. Concerning the uſe of reſpiration, therefore, we may reaſonably conclude, that an aerial ſomething, whatever it may be, abſolutely neceſſary to ſupport life, paſſes into the maſs of blood; thence air thrown out from the lungs, from which the vital particles are exhauſted, is no longer fit to ſerve the purpoſe of reſpiration. Mayow ſeldom hazards an opinion without the rigid teſt of experiment, and here he adduces one in proof of his doctrine.

(C) If the lungs of a dog be inflated by a pair of bellows, in ſuch a manner that the air may paſs through perforations made in them, which is immediately to be ſupplied by the bellows, leſt the lungs ſhould collapſe; in this caſe, I ſay, the dog will live. Yet when the lungs are kept in this conſtant ſtate of inflation, no ſhaking or agitation of the blood can take place; and we know too, that if the motion of the lungs be entirely ſuppreſſed, the blood will, nevertheleſs, paſs to the left ventricles. Again, if after a full inſpiration, the mouth and noſtrils be ſtopt, although the lungs remain inflated, the animal muſt die, becauſe it cannot make an exſpiration; and yet, in this caſe, the blood has a paſſage through the lungs, equally as free as in the former; nor can it be ſaid, that in the firſt inſtance, there was a greater breaking down of the blood, ſince in each experiment the lungs were equally diſtended, a decided proof, that reſpiration is not ſo neceſſary, either to the paſſage of the blood through the lungs, nor to break it down, ſince, in the firſt caſe, the animal lives, but in the laſt it dies, becauſe in that there was an occaſional ſupply of freſh air, but in this there was none.* No other concluſion could be drawn from theſe experiments and obſervations, than that which Mayow made; the inferences were perfectly juſt, and warranted the opinion founded upon them. Having thus proved, that reſpiration is not intended to break down the blood, nor to paſs it through the lungs, but that this function is intended to tranſmit air into the lungs, let us next inquire, ſays Mayow, what that aerial ſupply is, which is ſo abſolutely neceſſary to the ſupport of life, that we cannot exiſt one moment of time without it; and this he determines to be the fire-air particles. Here again, another difficulty preſented itſelf, which he was determined to ſolve, *viz.* ſince the fire-air particles are ſo neceſſary to exiſtence, what purpoſe do they anſwer, after being tranſmitted into the maſs of blood? Not content with having proved the brilliant truths we have detailed, he is reſtleſs, until he finds out, of what uſe the fire-air particles are to the ſyſtem. After having thus introduced the nitro-aerial particles into the blood, we muſt next inquire, what purpoſes they fulfil; and indeed, it is our opinion, that the nitro-aerial particles are the principal ſource of life and motion in animals. This life and motion, he proves, are produced by heat evolved in the ſyſtem from the fire-air particles. We ſhall preſently ſee how beautifully this obſervation is confirmed, and how exactly conformable Mayow's explanation is to doctrines lately promulgated.

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* An experiment, nearly ſimilar, has been made, by a celebrated modern phyſiologiſt, whoſe death ſcience muſt regret, to confirm an opinion he had formed, with reſpect to the cauſe of death in drowning.—Hunter, p. 118. See alſo Hales, Vol. I. 253.

It will be neceſſary, however, before we proceed any further, to ſay a few words on the modern doctrine of animal heat. The atmoſphere, it is well known, is compoſed of two kinds of fluids, azot and oxygen, which are kept in the gaſeous ſtate by caloric. During reſpiration, or the paſſage of the blood from the right to the left ſide of the heart, a great quantity of oxygen is abſorbed, rendering the venous blood arterial. Carbonic acid gas, and water, are formed during the proceſs; hence the blood loſes its hydrocarbonous impregnation, and has its capacity for caloric increaſed.* The oxygenated blood thus paſſes on to the different parts of the ſyſtem, through the medium of the circulation. During this courſe, but particularly in the capillary veſſels, a chemical change takes place; the oxygen unites with the other elementary principles, by which caloric is evolved, and the phænomena of animal heat produced. This, we think, cannot be effected by mere chemical affinity; it is evident, that a certain action of the veſſels muſt take place to produce the effect, and the conſequent change of caloric, from a latent to a ſenſible ſtate, muſt ariſe from a peculiar power, modified by the operation of the vital principle; hence the local heat that very often occurs in different parts of the body. In fevers too, where the action of the vaſcular ſyſtem is greatly increaſed and deranged, the diſengaged heat is one of the moſt troubleſome ſymptoms to the patient, and to leſſen this evolution of caloric, by whatever means it may be effected, is a great ſtep towards a cure; hence the good effects that have been produced by the effuſion with cold water, and which are certainly not to be explained from the mere reſtoration of tone. It is by no means our intention to aſſert, that there is a power inherent in the body, capable of producing heat, independent of the oxygen abſorbed by the lungs. Something very like this is maintained by the ingenious Dr. Currie, in his valuable publication on fevers.* He ſays, the vibrations of the arterial ſyſtem (in an experiment which he made) were unuſually ſlow, yet animal heat was diſengaged with more than four-fold rapidity.† This obſervation is made to refute an opinion maintained by ſome, *viz.* that animal heat is connected with the action of the heart and arteries. No inſtance, we believe, can be pointed out, where increaſed action of the heart and arteries is not attended with a proportional increaſe of the heat of the ſyſtem. The reverſe of the propoſiton, however, is not true, *viz.* if the action of the heart and arteries be diminiſhed, it will be followed by a proportional decreaſe of animal heat; this is very eaſily explained. It is well known, that the extreme veſſels have an oſcillatory motion, independent of the action of the heart and larger arteries, as has been proved by Whytt and De

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* To do juſtice to Dr. Duncan, the ingenious Profeſſor of Edinburgh, we muſt obſerve, that ever ſince the year 1774, he has delivered at his Lectures, a theory ſimilar to this; but in explaining animal heat, before the new diſcoveries, he employed the term phlogiſton, for which he has now very properly ſubſtituted the hydrocarbonous principle. The coincidence of opinion between the Profeſſor and Mr. Seguin, may be ſeen, by conſulting La Médicine Eclairée, Vol. I. p. 56.

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* Vide p. 177.

† Appendix, p. 35.

Gorter;* the pulſe is therefore no indication of the action that is going on in theſe capillary veſſels, and it is in theſe, we think, that the evolution of caloric is principally produced; hence more ſenſible heat is indicated by the thermometer in the right than in the left ventricle of the heart.† The ſubſtances taken into the ſtomach, will, no doubt, vary the actual degree of heat in the ſyſtem; for in proportion as theſe contain their component parts in a looſe ſtate of combination, ſo will they more readily unite with oxygen, and evolve the caloric; hence the heat produced by ardent ſpirits, but more evidently by æther, which has the hydrogen and carbon very looſely combined. We might carry our ſpeculations further, did our ſubject admit of it;‡ but it is time to return to Mayow. (A) In order to explain his ideas of animal heat, our ingenious author ſuppoſes, that a vital motion, or fermentation, takes place in the blood, by the union of fire-air with the ſalino-ſulphureous particles. Here he points out an analogy between the animal and vegetable kingdom; for as the nitro-aerial particles, which are abſorbed by the ſoil, ſtir up a gentle and gradual commotion with the ſalino-ſulphureous, upon which vegetable life depends, in like manner, the ſame nitro-aerial particles, being introduced into the maſs of blood by reſpiration, and being ultimately blended with the ſalino-ſulphureous, produce an efferveſcence of ſuch a nature, as is conducive to animal life. The dark and duſky colour of venous blood, is owing to the ſalino-ſulphureous particles;* and as fermentation in general depends upon the nitro-aerial particles, I do not heſitate to ſay, that the motion of the blood is derived from the ſame ſource. This opinion is confirmed, when we conſider, that the blood enters the lungs of a dark colour, but returns of a bright red, as has has been proved by the ingenious Lower. Here Mayow adduces the arguments and experiments of this phyſiologiſt in confirmation of this fact, and adds, that if the ſurface of the blood in a cup be rendered fluid by the air, we muſt not wonder that that effect ſhould be produced in the lungs, ſince the air, in this caſe, is diffuſed through every particle of blood, and intimately blended with it. That arterial blood contains more air than venous, is evident from the following experiment.

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* Whytt's Phyſiological Eſſays; De Gorter, De Motu Vitali.

† Coleman on Suſpended Reſpiration, p. 42.

‡ Is it not probable, that the coldneſs always felt in digeſtion is cauſed by the dyſpeptic ſtate of the ſtomach, forming ſubſtances, which, by having a greater capacity for caloric, abſorb, inſtead of evolving animal heat?

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* We wiſh the reader here to recollect, that the dark colour of the blood was attributed, till lately, to phlogiſton, for which we have now ſubſtituted hydrogen and carbon. Vide Crawford, p. 150.

(E) If blood which has been kept in a cup for ſome time, be placed in the air-pump, ſmall bubbles will riſe from its ſurface by exhauſting the receiver; but if arterial blood, yet warm, be placed in a ſimilar ſituation, it will expand in a wonderful manner, and throw out an infinite number of bubbles. We muſt here obſerve, ſays Mayow, that as the nitro-aerial particles cauſe the fermentation of the blood, ſo the fermentation ſtrikes out, and attracts the nitro-aerial particles from the air, by which the motion in the blood is again renewed; that is, the nitro-aerial ſpirit, mixed with the maſs of blood in the lungs, produces in it a fermentation ſufficiently ſtrong; the greateſt part, however, of theſe particles being ſecreted from the blood for purpoſes in the ſyſtem hereafter to be mentioned, the motion of the blood returning to the lungs, on account of their deficiency, is much diminiſhed and rendered ſlower: the fermentation, however, does not ceaſe ſo much, but that it can be again renewed by an additional ſupply of fire-air particles; and thus it is, that in the animal automaton, the motion of fermentation obſerves one continual round. In like manner as the motion, ſo does the heat of the blood depend upon the union of fire-air with ſalino-ſulphureous particles. For if any ſalino-ſulphureous mineral, as the vitriolic marchaſite, be expoſed to moiſt air, a heat and motion will be produced by the effervescence of the fire-air with the ſalino-ſulphureous particles. How much greater then muſt be the heat and motion of the blood which abound with the ſalino-ſulphureous particles properly prepared, and with which the fire-air particles are intimately blended by the power of reſpiration? In confirmation, I add, that the great heat obſerved in animals, from violent motion, takes place, becauſe reſpiration being very much quickened, a larger quantity of fire-air particles are thrown into the blood, by which a greater than uſual heat and motion are produced: in violent exerciſe we cannot artribute ſo much heat to the friction of parts, and indeed, if any one at reſt will breathe quick for awhile, he will feel a glow of warmth to pervade his whole body. How has this doctrine been thrown away, ſays Beddoes, upon Boerhaave and his diſciples, even upon Haller, the beſt of them all. Notwithſtanding that Mayow makes uſe of the word efferveſcence, he takes care to explain his meaning, and does not admit of that expanſion in the blood to which the action of the heart was attributed, previous to the Harveian diſcovery. He obſerves, that he by no means allows, that ferment, which has been ſaid to be preſent in the left ventricle, for part of the blood in the fœtus, does not enter the left ſide of the heart, but paſſes directly through the canalis arterioſus in the aorta, which would not be the caſe if a ferment was ſo neceſſary to the blood in the left ventricle; how much leſs probable then is it, that the pulſation of the heart depends upon a rarefaction, and expanſion of the blood in its ventricles. (F) If this were the caſe, the heart ſhould be conſiderably dilated at every pulſation, as a bladder is rendered larger by inflation; but it is evident, from diſſecting living animals, that the heart contracts at its pulſation, and the blood is expelled during contraction, and by no means during dilatation. Moreover, it appears, that the motion of the heart does not depend upon the rarefaction of the blood, for hearts cut from animals are obſerved to pulſate, although all the blood be ſqueezed out of them. Indeed I know, ſays Mayow emphatically, from repeated experiments, that if a ſolution of opium or cold water be injected into the jugular vein, the motion of the heart becomes quicker, which certainly does not ariſe from the expanſion of the blood. Whence it would appear, that the heart is a muſcle, which does not differ in its action from any other muſcle. The effervescence therefore which Mayow meant, as he expreſsly aſſerts, was not an expanſion or rarefaction in the blood, but a motion which was communicated by the air to the blood in the lungs, and which was continued as well in the whole vaſcular ſyſtem as in the heart. As this vital motion in the blood depends upon the abſorption of fire-air by the lungs, it was an inenevitable concluſion, that from a ſuppreſſion of reſpiration, the blood would loſe thoſe properties, by which it gave vigour and activity to the whole frame. But the blood is expelled with ſufficient force from the right venticle, when it is not yet impregnated with air. Blood, therefore, with different qualities, is equally capable of motion, ſince we find that in one ventricle it has acquired air, and, in the other, it is devoid of it. How then does it happen, ſays Mayow, that death ſo ſoon takes place from ſuppreſſed reſpiration, ſince blood without air, as happens in the right ventricle, is capable of motion? He explains himſelf in the following manner. (G) Life conſiſts in a diſtribution of the animal ſpirits; to ſupply which, it is abſolutely neceſſary that the heart ſhould pulſate, and that there be a continual afflux of blood to the brain; for it appears, that the motion of the muſcles, and that of the heart, depends chiefly on this aerial ſalt. But it is neceſſary to obſerve,

that

that the ſudden contraction of muſcles is produced, by the union and mixture of two kinds of particles, the one derived from the air, by reſpiration, the other ſupplied from the maſs of blood. Wherefore, when reſpiration is ſuppreſt, this aerial ſalt, ſo abſolutely neceſſary to motion, is withheld; the pulſation of the heart, and conſequently the afflux of blood to the head, muſt be interrupted, and death enſue. That this is the uſe of reſpiration, is further confirmed, by a variety of circumſtances. In the more violent exerciſes, we are obliged to breathe more quick and laborious; not that the blood may have a freer paſſage through the lungs, for we have ſhewn, that this can take place without their motion; but ſince, from the various effervescences produced in the action of muſcles, a very great waſte of the nitro-aerial ſalt is cauſed, ſo that the venous blood returns to the heart, very much impoveriſhed and grumous (as is obſerved more evidently in epileptic convulſions), therefore that the impoveriſhed blood may reſtore its waſte, there is a neceſſity for quicker reſpiration. Beſides, in the violent motions, on account of the more abundant flow of blood, the pulſations of the heart are increaſed, which could not be continued, (eſpecially as the blood is now deprived of air), without a more plentiful ſupply of nitro-aerial ſpirit : *hence we may conclude, that the chief uſe of reſpiration is, to cauſe and keep up the motion of the heart and muſcles.* We may further add, that if after the motion of the heart has ceaſed, on account of ſuppreſſed reſpiration, air be blown through a tube, inſerted in the *vena cava*, the motion of the heart will be gradually reſtored ; whence it appears, that the air is that property, without which the motion of the heart cannot be maintained : nor is it of any conſequence, by what means air is introduced into the maſs of blood ; whether through the lungs, or in any other way*. The doctrine which Mayow has thus laid down, is ſo exactly ſimilar to the theory ſuggeſted by the late diſcoveries, and

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* This nice obſervation has been confirmed, by an experiment of ours, in which the motion of the heart was kept up, independent of reſpiration, oxygen being ſupplied to the blood, through the coats of the veſſels. Needham, in the laſt century, reſtored to life a ſtrangled dog, by inflating the thoracic duct with air.—Vide infra, P.

is ſo beautifully illuſtrated by himſelf, that it ſcarcely requires a comment. That the blood acquires a vital air in the lungs, for the purpoſes of animal heat, and loſes it again, on its return to the right ventricle, is now, we believe, not doubted by any. That this idea impreſt itſelf, with full force, on the mind of Mayow, we think every one will allow, after what has been delivered. So convinced, indeed, was he, of the truth of his doctrine, and ſo clearly did he ſee into the operation of his fire-air particles, that he extends their influence, to various functions in the animal frame. Although he does not ſeem to have diſtinctly underſtood, how ſubſtances paſt from an aeriſorm to a condenſed ſtate, yet he ſaw and proved, the abſorption of fire-air, by the blood, in the lungs.

One apparently great objection to his doctrine, however, ſtill remained to be refuted, which was, that the fœtus in utero, and chick in ovo, lived, were nouriſhed, and acquired growth, without the function of reſpiration. In the explanation of this ſeeming contradiction, we ſhall ſee Mayow purſuing one uniform and ſyſtematic plan, and applying his doctrine, in a moſt beautiful manner. The moſt obvious, and therefore the moſt commonly received opinion, with reſpect to the uſe of the veſſels, was, that they were formed, for the ſole purpoſe of conveying nouriſhment from the mother to the child. Mayow denies, that this can be the ſole object of their formation; and refutes a variety of other opinions, brought forward on the ſubject. (H) Wherefore, ſince the functions, which have been aſſigned to the umbilical arteries, do not ſeem to be conſiſtent with the truth, we may be allowed to conclude, with the divine old man, Hippocrates, that the umbilical veſſels ſupply the office of reſpiration to the fœtus, which is alſo the opinion of the learned Everard*. We may, therefore, conclude, that the fœtal blood, conveyed through the umbilical arteries to the placenta, not only carries along with it a nutritious juice, but alſo a ſupply of nitro-aerial particles to the fœtus; whence the

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* Although Mayow thus agrees with the premiſes of Everard, he does not aſſent to his concluſion, which is, that the circuit of the blood, through the long and tortuous courſe of the umbilical veſſels, is intended to cool it.

blood of the fœtus, by circulating through the umbilical veſſels, becomes impregnated with nitro-aerial particles, in the ſame manner as in the lungs. We muſt, therefore, no longer denominate the placenta, the liver, but the lungs of the uterus.

That blood is only of uſe, in proportion as it is impregnated with fire-air, is a fact which evidently arreſted the attention of Mayow. He ſaw, that reſpiration was quickened in violent motion, for the purpoſe of ſupplying that which was expended in the different functions; hence he concluded, that the fœtus muſt derive this aerial ſupply from ſome ſource. (I) Indeed it is very probable, ſays Mayow, that if arterial blood, which is impregnated with the nitro-aerial ſpirit, could be conveyed to the heart, inſtead of venous, there would be no occaſion for reſpiration. In confirmation of this, he mentions a very curious and remarkable experiment, which, if juſtly appreciated, cannot fail to convince the reader, that Mayow's ideas, with reſpect to the agency of fire-air in the body, are exactly ſimilar to thoſe of the moderns. (K) Whilſt arterial blood is tranſfuſed from one dog into another, although the dog, into whom the blood is injected, is panting and breathing deeply, yet upon receiving the arterial blood into his veins, he ſcarcely ſeems to breathe. The animal having received, ſays the ingenious Beddoes, from an unuſual ſource, the ſubſtance which is probably expended in muſcular action, it was, therefore, not neceſſary to inhale it rapidly.

The diſcovery made by Mayow, that fire-air is abſolutely neceſſary to the fœtal œconomy, and the ſource from whence it is derived, have now, we believe, no opponents. This function of the umbilical veſſels, Mayow extended to the chick in ovo. It is our opinion, he ſays, that the chick breathes through the umbilical veſſels, in a ſimilar manner as the fœtus in utero. If it ſhould be objected, that the air contained in the liquors of the egg, is not ſufficient to ſupply reſpiration, during the whole time of incubation, I anſwer, that the air contained in the egg is not common air, but that aerial matter, ſelected from the common air, by the power of reſpiration. Beſides, the fœtus in utero and ovo, have occaſion for only a ſmall quantity of nitro-aerial particles, becauſe the fœtus, in either caſe, has no motion, except that of the heart; therefore a ſmall ſupply of nitro-aerial particles, from the arterial blood of the mother, and the liquors of the egg, is abundantly ſufficient for the purpoſe. The gentle warmth, too, excited in the egg by incubation, ſupplies the place of nitro-aerial particles, upon which depends the heat of the ſyſtem: for as the nitro-aerial particles enter the earth, and effervefce with the ſalino-ſulphureous, from which are derived the reſpiration and life of vegetables; and as the ſame particles, mixed with the blood in the lungs, cauſe a motion neceſſary to animal life; ſo being blended with the liquors of the egg, by the influence of the gentle warmth, they induce a vital fermentation, and animal motion in them, and ſupply the place of reſpiration. We are at a loſs, which moſt to admire, the beautiful facts Mayow diſcovered, or his cloſe and acute reaſoning upon them. Nothing can be more conſiſtent, than the manner in which, he ſays, fire-air is ſupplied to the fœtus in utero and ovo; and the reaſon he gives, for the fœtal ſtate not requiring ſo much fire-air, is admirably explained by him, as the reader will ſee in the ſection on muſcular motion.

SECT. VII.

OPINIONS *of* AUTHORS, *contemporary with* MAYOW, *concerning* RESPIRATION.

About the time, that the ingenious Mayow gave to the world his ideas of reſpiration, two treatiſes were publiſhed, on the ſame ſubject; the one by Swammerdam, the other by Thruſton. It was the opinion of Swammerdam, that ſomething was mixed with the blood, during reſpiration, and which was neceſſary to its perfection; and alſo, as the blood paſt through the lungs, vapours were thrown off, as noxious to the ſyſtem. He aſks himſelf the queſtion, Of what uſe is reſpiration to life? and he anſwers, that it is of utility to the heart, and that, for the ſake of the preſervation of life, which we think conſiſts in the final perfection of the blood, in the left ventricle of the heart. He maintained, that the more ſubtile part of the air was abſorbed; and by his expreſſions ſeems to think, that the vapours which are exhaled, make room for the abſorbed air. He held, nevertheleſs, the common opinion of the times, that the inſpired air was intended to cool and condenſe the blood, become heated and rarefied by fermentation, in the right ventricle. He mentions, however, that the heart is ſtimulated to action, and nouriſhed, by the fine part of the air, taken in during reſpiration. Much confuſion and perplexity of ideas pervade the whole of his treatiſe; and he ſeems occaſionally at a loſs, to find terms to convey to the mind of the reader, his notions of the effect of air on the blood, reſulting, no doubt, from a want of experiment, by which alone, a knowledge of this kind is to be acquired. He relates, however, a curious experiment, which he made. A long flexible tube was fixed in an opening, made in the trachæa of a dog: the animal was then immerſed in water, and he lived for a conſiderable time, by breathing through this tube, the upper extremity of which roſe above the water, on the ſurface of which, the reſpiration of the dog produced a beautiful undulation.

In Thruſton's publication, we find ideas very ſimilar to thoſe we at preſent entertain, with reſpect to the uſe of reſpiration.* He ſeemed to be aware of the great connection between air and life, and of the dependence of the one upon the other, and quotes, in confirmation of this, the paſſage in Geneſis, ‘ He breathed into his noſtrils the breath of life.’ In order to ſhew in what manner reſpiration is ſo neceſſary to animals, he firſt conſiders motion as abſolutely indiſpenſable to the blood, while it is poſſeſſed of vitality. He brings forward the arguments of Harvey, for the circulation; and maintains, that the cauſe of it is heat, derived from the air abſorbed by the blood in the lungs. He makes ſome obſervations upon the nature of air, which, he ſays, poſſeſſes nitrous particles, for which he refers us to Gaſſendi, Ent, and Digby. It is very probable, ſays Thruſton, that as the more pure, more ſubtile, and more penetrating part, is abſorbed by the lungs, and is there, as it were, loſt, ſo the expired air, becoming replete and vitiated with the exhalations, and deprived of its better part, is, in ſome degree, rendered effete and uſeleſs: moreover, I believe, it loſes, very conſiderably, its elaſticity, being, in a great meaſure, deprived of its more active matter. How ſimilar, with ſome exception, is this, to the reaſoning of Mayow. Were the experiments of Mayow, before publication, noiſed abroad, ſo as to ſuggeſt theſe ideas to Thruſton; or did opinions, like theſe, beginning to prevail at that time, from obſervation, rouſe the genius of Mayow, to inſtitute experiments to confirm the truth of them? Certain it is, that at this period, immediately, after Charles was reſtored to the crown, the genius of medical men began to enquire, more minutely, into the ſecrets of nature. Harvey had but juſt immortalized his name, by the diſcovery of the circulation; Sydenham was in all the glory of his medical career; and the different branches of medical ſcience were enriched, by the elegant publications of Willis, Gliſſon, Mayow, und innumerable others. When this noble, but alas! much abuſed profeſſion, was adorned by ſuch able ſupporters, every attention would be paid to the phænomena of the animal œconomy, and every theory would be brought forward, that induſtry could examine, and abilities explain; and it appears, that a hiſtory of the ſtate of phyſic, during the laſt century, would afford an intereſting account to the medical reader.—— But to return.

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* I cannot avoid remarking, as very ſingular, that in every work I have conſulted, publiſhed in the time of Mayow, no notice is taken of him. Thruſton's work was originally a theſis, which he wrote for his degree at Cambridge. It is a cuſtom in the Univerſities of Cambridge and Oxford, for the candidates to write a paper, on a medical ſubject, in which they are queſtioned by the Profeſſors, in the preſence of a numerous audience. Thruſton's theſis was firſt made known, when he took his degree, in 1664. Anno enim 1664, Cantabrigiæ in ſcholis medicis circumfuſa, virorum doctiſſimorum et juventutis florentiſſimæ, frequenti corona comparuit. Mayow's treatiſe was publiſhed about four years after, *viz.* in 1668; but Thruſton's was not delivered to the public, for ſeveral years after he took his degree: yet he makes no mention of Mayow, notwithſtanding he maintains, that reſpiration is to warm the blood; an opinion firſt proved by Mayow, in his beautiful experiments, and in oppoſition to the prevailing theories of the times.

The intromiſſion of air, into the blood, by the lungs, according to Thruſton, was neceſſary to its fluidity and heat. (M) The particles of the air, being mixed with the blood, conduce not only to its fluidity, but alſo to the preſervation of its heat. The heat is owing to the nitrous particles of the air, which, he ſays, he could eaſily prove, by a variety of experiments, unleſs he ſtudied brevity; and to explain its *modus operandi*, in producing animal heat, he has recourſe to fermentation. It was obſerved, too, by him, that the blood acquired its florid colour, by its union with air; whence it is rendered more fit for the functions of the circulation. His theory had been taken notice of by ſome, without mentioning his name; and he adds, it is with peculiar pleaſure I find, that the experiments of Lower confirm my ideas. (N) It is our opinion, that this is the peculiar office of the lungs, *viz.* to tranſmit air into the blood, by which it acquires a red and florid colour; whence it happens, that it becomes not only more florid, but more fluid; and the more fluid it is, the more eaſily will it be fitted for the functions of the circulation. Thoſe two celebrated philoſophers, Hook and Lower, ſeem to have placed this matter beyond a doubt, by a very ingenious experiment. And I do not a little rejoice, and return them my hearty thanks, that they have produced a complete confirmation of my theory, which has been publicly attacked by ſeveral, without mentioning my name. It was an opinion, held by medical men, for ſome time after the diſcovery of the circulation, and till the nature of reſpiration became better underſtood, that ſanguification was performed by the heart alone. This is entirely rejected by Thruſton: the office of the heart, he ſays, is to circulate and diſtribute the blood, through all parts of the body, and with it, the vital heat. If we at all conſider the nature of ſanguification, and examine into the effects produced in the blood, we ſhall not fail to attribute them to the lungs. No phænomena in the ſyſtem, ſince the late diſcoveries in chemiſtry, have excited greater attention, than the ſuſpenſion of reſpiration, in drowning, hanging, and ſuffocation. It is generally believed, that the cauſe of death, in theſe caſes, has been but lately diſcovered. Various have been the conjectures offered, and as various the publications. It is evident, that unleſs medical men had a true idea of the function of reſpiration, no adequate explanation could be given, of the cauſe of death, in hanging, drowning, and ſuffocation.

Among the writers who have adorned phyſyology, by their publications on this ſubject, that of the ingenious Mr. Coleman ſeems to have been conducted with moſt accuracy.* This gentleman has ſhewn, that irritability is more or leſs connected with animal heat, which the blood acquires in its paſſage through the lungs; that the difference of irritability in the two ventricles, is owing, in drowning, hanging, &c. to the blood having loſt its heat, in conſequence of not undergoing the neceſſary change in the collapſed lungs. It is moſt aſſuredly true, that the blood in the right ventricle, during ſuſpended reſpiration, muſt poſſeſs more heat, than when it arrives at the left; becauſe it has traverſed through the lungs, where it muſt have loſt a conſiderable quantity, to ſupport the life of thoſe organs. The proximate cauſe of death, in the caſe of drowning, &c.

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* To do juſtice, however, to Dr. Fothergill, of Bath, we muſt ſay, that from ſome accidental circumſtances, we have not read his work on this ſubject; it is a pleaſure we promiſe ourſelves very ſhortly.

according to Mr. Coleman, is a want of latent heat in the blood, with obſtruction in the pulmonary veſſels, from the collapſed lungs. It appears to me, that want of the latent heat alone, is the proximate cauſe. I regard the want of latent heat, as an effect produced by the collapſed lungs. Mayow, and others, have long ago ſhewn, that fluids can paſs from the anterior to the poſterior cavities of the heart, when the lungs are collapſed. The only intention of nature, in diſtending the lungs, during reſpiration, is, that more points of contact may take place, between the blood and atmoſpheric air, for the abſorption of oxygen; and were it poſſible, that the ſame quantity of oxygen could be abſorbed by the lungs, when they are collapſed, as in the other caſe, I have no doubt, that there would be no occaſion for the alternate contraction and dilation of the thorax. I ſpeak abſtractedly of reſpiration, for every one allows the utility of preſſure, &c. in reſpiration, producing conſiderable effects, in promoting the flow of the fluids of the body.

The ſame idea, with reſpect to the cauſe of death, in drowning, &c. preſented itſelf to the ingenious Thruſton; and probably Mr. Coleman will be ſurpriſed to find his theories and experiments, in part, anticipated, as far back as the year 1664. It occurred to Thruſton, that the death produced by hanging, drowning, and ſuffocation, aroſe from one and the ſame cauſe. Many and various experiments had fully proved, that in theſe caſes, air was prevented from uniting with the blood; hence its motion was ſtopped, and life deſtroyed. He had obſerved, that the anterior cavity was always greatly diſtended with blood; for from the ſuſpenſion of reſpiration, there was nothing which could convey that principle to the blood, by which it acquired its heat, fluidity, and vitality. (O) Having premiſed thus much, ſays Thruſton, it is ſcarcely neceſſary to enquire, why animals die ſo quickly, in hanging, drowning, and ſuffocation: we muſt repeat, that air being withheld from the blood, its motion ceaſes, and life is deſtroyed. That this is the caſe, is proved by a variety of obſervations and experiments. Harvey ſays, that in a body juſt hanged, he found the right auricle and lungs very much diſtended and infarcted with blood*. Reſpiration ceaſing, there was nothing which could propel the blood into the left ventricle; nothing which could properly blend it together, and communicate to it vitality, fluidity and heat. Experiments were inſtituted, to ſhew how neceſſary the preſence of air is to the vitality and motion of the blood. Hook had, before this, exhibited to the Royal Society, the experiment of affording and with-holding life, by the motion of a bellows in the trachea of a dog; and Profeſſor Croon, at London, before the Preſident and Fellows of the College of Phyſicians, reſtored to life a ſtrangled fowl, by blowing air into its lungs. Dr. Needham too, at Oxford, in the preſence of Boyle and others, recovered a dog, which had been hanged, by inflating the thoracic duct with air. (P) The *inſpired* air, obſerves Thruſton, united with the blood, and promoted its fluidity, by which means it reſtored the circulation, which had ſtagnated in the right ventricle, and in the lungs. He adds, that bleeding is often found of

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* Epiſt. ad Riolanum.

uſe, in caſes of hanging, &c. and it could only produce its good effects, by relieving the right ventricle and lungs of their load of blood; hence the great uſe of that remedy, in ſome caſes of difficult reſpiration. The remarks which Thruſton has made on this ſubject, are very judicious; and the coincidence, which had taken place, between his obſervations, and thoſe of Mr. Coleman, is curious and worthy of notice. The heart has been found, ſays this gentleman, to be loaded. If, therefore, from the right ſide of the heart, while thus in a ſtate of violent plethora, a ſmall quantity of blood could be taken, experiment and obſervation tell, that its power and actions would be inſtantly re-invigorated*.

Although it had been aſſerted and proved, by many, that the preſence of air, was abſolutely neceſſary to the life of animals, yet ſeveral very able men denied, altogether, its union with the blood in the lungs†. It was urged, that no experiments had decidedly proved it; that the ſame pores which gave entrance to the air, would ſuffer the blood to eſcape. Willis, however, imagined, that the air got acceſs to the blood, immediately before it paſſed from the artery into the vein, when it is changed from a dark to a florid colour*. They did not know, that air could unite with blood, through a medium as denſe as a bladder. Experiments had not been made to prove the abſorption of air by the blood, when Harvey and Highmore denied it; and, had Thruſton attended to the experiments of Mayow, he would have been able to have urged more powerful arguments, in favour of the abſorption of air. Lower too, ſoon after, as we ſhall preſently ſee, inconteſtibly proved, by his experiments on animals, the union of air with the blood in the lungs.

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* P. 163-4.

† Harvey, Highmore, Diemerbroeck, &c.

Notwithſtanding,

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* Pharmaceut. Rat. P. 17. By the way, we may obſerve, that this ingenious phyſician maintains, that the uſe of reſpiration is to give vitality to the blood, by its union with air. He takes notice of the difference between arterial and venous blood; and ſays, that the blood, in paſſing through the lungs, to the pulmonary vein, is animated by the air; and adds, that this vein is exactly ſimilar to an artery, and that you may inject it from trunk to branch.

Notwithſtanding, however, that Mayow's experiments had decided, beyond the ſhadow of the doubt, that atmoſpheric air is compoſed of two kinds of air, the one the preſerver, the other the deſtroyer of animal life; and Lower had clearly ſhewn, by additional experiments, its abſorption by the lungs; yet we find an ingenious anatomiſt denying its union with the blood; ſo much does prejudice pervert the judgment. How air could be brought from the elaſtic, to the unelaſtic ſtate, was a difficulty, which could not be ſolved by the philoſophers of the laſt century; and they did not attempt to explain it; becauſe they never imagined ſuch a change. From different facts and obſervations, they could underſtand, how air might *mix* with the blood in the lungs; but beyond this, they could not extend their ideas, as the powerful and neceſſary aid of experiment was wanting. They ſaw the fœtus in utero, living and growing without reſpiration; they obſerved, that all the blood did not paſs through the fœtal lungs, and that nature, to ſupply this defect, had formed other channels, to tranſmit the blood from the right to the left ſide of the heart, *viz.*

the foramen ovale, and canalis arterioſus. Hence they concluded, that as air could not arrive at the fœtus, and that as the temporary channels were obliterated in the adult, reſpiration was of no other uſe, than to circulate the blood through the lungs. It was thus imagined, that the moſt powerful argument againſt the abſorption of air by the lungs, was the event of reſpiration in the fœtus in utero. (Q) If air be ſo neceſſary to the adult, why ſhould the fœtus be deprived of that privilege? But in the fœtus, no union of air can take place, becauſe it cannot breath in the uterus; yet in it, equally good and fine blood is prepared as in the adult who breathes.

Of this objection, Thruſton was aware; and although he mentions that the mutual heat and umbilical veſſels ſupply the place of the lungs and reſpiration, yet he ſeems to lay hold of the idea, leſt he ſhould be obliged to give up his theory, of the abſorption of air by the lungs, and not as if he underſtood the nature and office of the placenta. Had Diemerbroeck, with his uſual penetration, attended to the experiments and obſervations of Mayow and Lower, how ſoon would he have been convinced of his error? He would there have ſeen, that the placenta ſupplied the place of the lungs and reſpiration, by oxygenating the fœtus with the maternal blood. He only mentions Mayow once, and then in ſuch a manner as if he did not at all underſtand the ſublime truths he had diſcovered. Maurocordatus had publiſhed at Conſtantinople ‘ De Motu et uſu Pulmonum,’ in which he denied, that reſpiration was of any uſe, except to tranſmit the blood from the right to the left cavities of the heart. This opinion, ſays Diemerbroeck, John Mayow has refuted by a celebrated experiment. The experiment, I ſuppoſe, for he does not quote it, of injecting a fluid into the pulmonary artery, and finding it in the left ventricle.

The difference between the colour of arterial and venous blood, formed a no leſs ſubject for ſpeculation, than the function of reſpiration. Hippocrates knew nothing of the diſtinction between artery and vein, and confounded both under the name of φλεψ. Eraſiſtratus, who had been invited by the Ptolemies into Ægypt, where he made ſome diſſections, was the firſt who diſtinguiſhed veins from arteries, which he called ſmooth, in contradiſtinction to the aſpera arteria. He imagined, however, that they contained air. Galen next aſſerted, that arteries were not air veſſels, which he diſcovered by the obvious experiment, of paſſing ligatures round the veſſels. He obſerved, that arterial blood was differently coloured from that of the veins, which, he ſaid, was owing to the air, which mixed with it in the arteria venoſa. He imagined, however, that it united with the air, by paſſing through the ſeptum cordis. Columbus maintained, that the blood was mixed with air, in its paſſage through the lungs; and this was, alſo, the doctrine of Servetus, which he publiſhed in his book ‘ De Reſtitutione Chriſtianiſmi.’ Theſe obſervations, however, that blood acquired its colour from the air, were made without actual experiment, and was a mere theoretical opinion. Neither was it adopted by phyſiologiſts in general: and the doctrine which obtained the moſt credit among medical men was, that the colour of the blood was owing to the heat it acquired in the heart; little imagining, that both the heat and colour are originally owing to one and the ſame cauſe. The obvious experiment of obſerving the blood in the pulmonary artery and vein, had not *directly* occurred to any one, till the middle of the ſeventeenth century. The doctrines which began to prevail at this time, reſpecting the laws of the animal œconomy, bear a ſtriking analogy to thoſe which are at preſent adopted. It was an age, indeed, remarkable for accuracy of obſervation and elucidation of facts. Certain it is, that at this period, opinions, ſuppoſed to be modern, were whiſpered in literary circles, and ſuggeſted experiments, upon which to build the foundation of rational ſyſtems. The prejudices of mankind, and a ſuperſtitious veneration for the dead, were now conſiderably obliterated, by the diffuſion of knowledge. The neceſſity of the ſtudy of anatomy, was perceived by men not connected with the profeſſion, and whoſe minds had been emancipated from the ſhackles of prejudice, by a liberal education: hence diſſections became more frequent; and in proportion as theſe prevailed, diſcoveries were made, which refuted or confirmed the theoretical opinions of their predeceſſors.

Among thoſe who by their induſtry and attention, acquired and merited their medical celebrity, was the ingenious Lower. This phyſician, by his accurate and well conducted experiments, convinced the unprejudiced mind, at once, that the change produced in the blood, was owing to the abſorption of air. We are by no means, to imagine, that the blood derives its colour from the heat which it acquires in the heart. This opinion is ſufficiently refuted, by obſerving, that the colour is different in each ventricle; and ſince their functions are ſimilar, why ſhould not the change take place, equally in one as in the other? for every one will allow, that blood, drawn from the pulmonary artery, is venous, in every reſpect. (R) Experimental enquiry clearly proves to us, that the new redneſs is not acquired in the left ventricle; for if we inſert a cork into the divided trachea of an animal, and draw blood from the cervical artery, immediately upon ſuffocation, it will be completely venous, in every reſpect, as if it had flowed from an opened jugular. However, that no room might be left for doubt upon the ſubject, the following experiment was made on a dog, deprived of all ſenſation and motion by ſuffocation, to prove the effect of air upon the blood, in its paſſage through the lungs. (S) The lungs, which were perforated, were inflated by air blown into them; the event anſwered expectation completely; for the blood was received into a cup (from the pulmonary vein) equally as florid, as if it had been taken from the artery of a living animal.*

(T) The experiments which prove, that the blood acquires its red colour from aerial particles, is confirmed by the common obſervation, that the ſurface of venous blood becomes florid, when received into a cup, by being in contact with air. If this be removed by a knife, the next ſtratum will acquire the ſame colour, from a ſimilar cauſe. It is, therefore, very evident, that the bright red colour of the blood, is owing to the particles of air inſinuating themſelves into it; the whole maſs is rendered florid in the lungs, becauſe in them, the air being diffuſed through all the particles of the blood, is intimately blended with it; and if blood be kept conſtantly agitated, as it flows from a vein, it will become florid throughout. If you aſk, ſays Lower, in what manner the nitrous ſpirit of the air paſſes into the blood, do you ſhew me, by what pores the ſame nitrous ſpirit of ſnow, paſſes through veſſels and cools fluids*. (V) We may, therefore, rationally conclude, that the blood, in its paſſage through the lungs, abſorbs air, to which its red colour is altogether owing; afterwards, however, when the air has eſcaped from the blood, into the *ſtructure of the ſyſtem and parenchyma of the viſcera*, and has tranſpired through the pores of the body, it is perfectly agreeable with reaſon to ſay, that the venous blood, being deprived of it, appears, on that account, more duſky and black. Can any reaſoning be more cloſe or more accurately applied? or where ſhall we find induction, from experiment, better underſtood, or more clearly perceived? It is, however, remarkable, and rather ſingular, that Lower no where mentions his ideas, with reſpect to the *uſe* of the inſpired air. He contents himſelf with a ſimple relation of facts, and probably did not wiſh to hazard his reputation upon theory, particularly as there appears to have been a jealouſy entertained againſt Mayow, for the new doctrines which he had ſo boldly and ſo confidently aſſerted. It appears extraordinary, that Lower ſhould not have quoted Mayow's experiments and obſervations, in confirmation of his own opinion, that the blood loſt the florid colour it acquired in the lungs, in conſequence of the air being diſengaged from it in the ſyſtem, particularly too, when that opinion was very much doubted by phyſiologiſts of the firſt reſpectability.* Lower, however, throws out ſome obſervations upon the ſubject, which had long before partly occurred to medical men. (W) From obſerving the effects produced upon the blood, during reſpiration, we may eaſily conjecture, how much the health of the blood depends upon its union with the air, and of what conſequence it is to reſpire it pure; and how greatly they are in the wrong, who deny this intercourſe between the air and blood, without which it would be equally as poſſible to enjoy health in the filth of a priſon, as in the pleaſantneſs of country air. He adds this remarkable obſervation, which ſhews how gradually knowledge was ſtealing upon the mind,—for wherever a fire burns freely, there alſo we breathe with moſt eaſe. Little did Lower imagine, how intimately connected this obſervation was with the admirable experiments of the neglected Mayow, and how beautifully illuſtrated by them. Nothing can afford a more pleaſing amuſement to the rational mind, than to obſerve the growth of knowledge, in ſucceeding generations: and little praiſe is due to that man, whoſe mind would not grow warm from the contemplation of ſcience, or whoſe enthuſiaſm would not encreaſe, from diſcovering the beauties of an author. Surely the mind muſt be deeply intereſted, when ſhe obſerves the gradual diſplay which genius makes in the advancement of knowledge; and when ſhe beholds the efforts of aſpiring abilities ſtumbling upon truth, but again falling from it, for the want of experiment.

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* The lungs were perforated, that they might be conſtantly diſtended, and that the air might have free egreſs. Lower uſes the terms *floridus*, *purpureus*, *rutilus*, & *coccineus*, indiſcriminately.

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* This was a branch of the atomical philoſophy, which conſidered frigorific particles as the cauſe of congelation.

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* Lower's Treatiſe on the Heart was publiſhed ſeveral years before Mayow wrote; but the fourth edition of the Tractatus de Corde came out above ſix years after Mayow gave to the world his Experiments and Obſervations on Reſpiration.

(A) Τοσαύτη δε τυγχανέι πᾶσι χρειη τοῖσι σώμασι τȣ πνευματος ἐȣ͂σα, ὥστε των μεν ἄλλων ἁπαντων ἀπεχομενος ὁ ανθρωπος, και σιτίων και ποτων, δύναιτ' ἂν ἡμερας δυο και τρεῖς καὶ πλείονας διάγειν: ἢν δέ τις επιλάβοι τας τȣ πνεύματος εἰς το σῶμα διεξοδȣς, ἐν βραχεῖ μέρει ἡμέρης, ἀπόλλοιτο ἄν, ὡς μεγίστης χρείης εȣσης τω σωματι τȣ πνεύματος. De Flatibus.

(B) Imo certe conſtat, poſſe ſanguinem, ſine ipſorum motu pulmones pertranſire: etenim ſi ſanguis, aut liquor quivis alius, ope ſyringæ, in arteriam pulmonalem animalis mortui injiciatur, idem in ſiniſtrum cordis ventriculum prompté ſatis permeabit. Et utique quiſque in ſe experiri poteſt, quanquam ſuppreſsâ ad tempus reſpiratione, pulſum tamen arteriarum in carpis ſatis validum eſſe, quod vero non contingeret, niſi ſanguis, per pulmones, ad ſiniſtrum cordis ventriculum, tunc temporis tranſiret. P. 298.

(C) Nempe ſi ope follium aſperæ arteriæ

adaptatorum, animalis cujusvis, veluti canis, inſufflentur pulmones, ita tamen ut per foramina in eorum extremitatibus hinc inde facta, etiam exeat aliquid aeris, qui iterum per folles ſupplendus eſt, ne pulmones concidant, in hoc caſu vivet animal. Et tamen in pulmonibus incontinenter inflatis, iſtius modi, ſanguinis conquaſſatio locum habere nequit; quin et pulmonum motu prorſus ceſſante, ſanguis, nihillominus, per eoſdem, ad ſiniſtrum cordis ventriculum tranſmittitur. Sin autem alias, inſpiratione inſtitutâ, ſprituque in pulmones hauſto, os et naris alicui obturentur, quamvis inflati maneant pulmones, mori tamen neceſſe erit, quia non licet expirare. Et tamen, in hoc caſu, æque patet ſanguini tranſitus per pulmones, ac in priore, neque enim in illo, major eſſe poteſt ſanguinis comminutio, cum in utroque caſu, pulmones ſe pariter diſtentos habeant; certo utique argumento, neque ad ſanguinis tranſitum per pulmones, neque ad conquaſſationem, adeo neceſſariam eſſe reſpirationem; ideo autem in priore caſu vivit animal, in poſteriore tamen moritur; quia in illo novus eſt ſubinde aeris recentis acceſſus, in hoc vero nullus. P. 299-300.

(D) Nimirum quemadmodum particulæ nitro-aereæ, terræ ſpiracula lente ſubeuntes, ibidem cum particulis ſalino-ſulphureis, iis verô immaturis, æſtu obſcuro congredientes, a quo vegetabilium vita dependet; ita particulæ eædem nitro-aereæ, magis conſertim in cruoris maſſam pulmonum miniſterio introductæ, particuliſque ejus ſalino-ſulphureis, ad juſtum rigorem evectis, quoad minima admixtæ, fermentationem ſatis inſignem qualis ſc. ad vitam animalem requiſita eſt, efficiunt. P. 47. Cruoris maſſa a particulis ſalino-ſulphureis, ad juſtam volatilitatem evectis, conflatur, unde fit quod ei color atro-purpureus ſuppetat. Enimvero fermentationes rerum naturalium fere quaſcumque, a particularum nitro-aerearum motu procedere, et utique ſanguinis æſtum ab eâdem causâ provenire nullatenus dubito. P. 148.

(E) Si ſanguis in vaſe aliquandui ſervatus, in vitrum collocetur, ex quo aer per antliam aeream exhauritur, ſanguis iſte in ſuperficie, quâ idem colorem floridum obtinuit, leniter efferveſcet et in bullulas aſſurget. Sin autem ſanguis arterioſus adhuc incaleſcens, in loco aere vacuo poſitus fuerit, idem mirum in modum expandetur et in bullulas pene infinitas elevabitur. P. 149. Annotandum eſt hic loci quod ſicut particulæ nitro-aereæ ſanguinis fermentationem efficiunt, ita fermentatio iſta particulas nitro-aereas alias, ex aere excutit, elicitque, a quibus æſtus in ſanguine de novo inſtauratur. Nimirum ſpiritus nitro-aerei ſanguinis maſſæ in pulmonibus immixti, effervescentiam ſatis intenſam in eâdem excitant; iidem vero mox in corporis habitu maxima ex parte a ſanguine in uſus infra dicendos ſecernuntur; ita ut æſtus cruoris ad pulmones reverſi, ob eorundem defectum multum imminutus et remiſſius fiat; neque tamen fermentatio adeo penitus ceſſat, quin eadem particulis nitro-aereis ex aere pro ſui renovatione eliciendis valet. Atque ita demum in automato animali motus fermentationis perennatur. P. 150-1. Quemadmodum ſanguinis fermentationem, ita etiam illius incaleſcentiam a particulis nitro-aereis cum particulis cruoris ſalino-ſulphureis exæſtuantibus, oriri exiſtimo. Etenim ſi minera quævis ſalino-ſulphurea, cujuſmodi ſunt marchaſitæ vitriolicæ aeri humido exponantur, eædem brevi æſtum caloremque intenſum concipient; in quantum ſc. particulæ aereæ cum particulis mineræ ſalino-ſulphureis congreſſæ, efferveſcentiam inſignem excitant. P. 151. Quanto ergo major ſanguinis æſtus fervorque erit, qui particulis ſalino-ſulphureis, rite evectis abundet et quibus particulæ aereæ confertim et quoad minima, pulmonum miniſterio admiſcentur? Quibus inſuper addo, calorem iſtum adeo intenſum, quo animalia in motum violentum concita afficiuntur, ex eo partim provenire, quod in motibus violentis reſpirationem magnopere intendi neceſſe ſit; unde fit, ut particulæ nitro-aereæ majori copia in ſanguinem introductæ, efferveſcentiam, caloremque ſolito majorem efficiant: neque enim in motibus maxime violentis membrorum attritio tanta eſt, quæ calorem adeo fervidum excitare poſſit. Imo ſi quis etiam in quiete aliquantiſper intenſius reſpiraverit, idem ſe calore, ſolito majore, mox ſuffuſum eſſe ſentiet. P. 152.

(F) Etenim ſi cordis pulſatio a ſanguine in ſinubus ejus exæſtuante perficeretur, tunc pulſante corde, illius ventriculi a ſanguine iſto maxime dilatarentur; haud aliter quam veſica in figuram maxime capacem inflatur. At vero ex vivi ſectionibus conſtat, cordis ventriculos in pulſu ſuo contrahi et non a ſanguine rarefacto ſive exploſo dilatari, item ſanguinem e corde ſe contrahente, non vero reſoluto, proſilire. P. 302. Et ulterius adhuc conſtat motum cordis a ſanguine rarefacto non fieri, quippe corda nonnumquam execta, quanquam expreſſo ex ventriculis eorum ſanguine, nihilominus pulſare obſervantur. Enimvero ſi opii diſſolutio, aut aqua frigida per venam jugularem injiciantur, cordis motus ſtatim crebrior erit, ut ſæpe expertus ſum, at hoc propter crebiorem accenſionem fieri non poteſt. Plane ut cor nihil aliud eſſe videatur, quam muſculus, in actione ſua e cæteris parum diſcrepans. P. 303.

(G) Vita ni fallor, in ſpirituum animalium diſtributione conſiſtit, quibus ſupplendis cordis pulſatione ſanguiniſque ad cerebrum affluxu omnino opus eſt. Enimvero veriſimile eſt, ad quemvis muſculorum motum ſal hoc aereum omnino neceſſarium eſſe. P. 304. Etenim arbitrari fas ſit, ſubitam illam muſculorum contractionem a diverſi generis particulis invicem commixtis procedere. Particulas nitro-ſalinas ex aere inſpirato hauſtas particularum motivarum alterum genus conſtituere, quæ particulis aliis ſalino-ſulphureis a ſanguinis maſſa ſuggeſtis, effervеſcentiam eam excitant, a quâ muſcularis contractio procedit. Quapropter ſuppreſſâ reſpiratione, cum ſal illud aereum, ad motum quemvis requiſitum, deficiat, cordis pulſationem et conſequenter ſanguinis ad cerebrum affluxum interrumpi, mortemque ſequi neceſſe erit, 304-5. Et hic reſpirationis uſus ulterius confirmari poteſt. Etenim in exercitiis, motibuſque violentis intenſiore utique, et crebriore reſpiratione opus eſt; non tam ut major ſanguinis affluxus liberius per pulmones tranſeat; hoc enim ceſſante reſpiratione fieri poſſe oſtendimus; ſed quoniam per varias iſtas effervеſcentias, in muſculorum contractione factas, maxima ſit ſalis nitro-aerei impenſa, ita ut ſanguis venoſus multum jam depauperatus et grumoſus ad cor redeat (uti etiam poſt motus convulſivos in epilepſiâ factos evenire certum eſt) quapropter ut ſanguis effætus diſpendia reſarciat, intenſiori reſpiratione omnino opus eſt. Præterea in motibus violentis ob uberiorem ſanguinis affluxum, cordis pulſationem citatiorem eſſe neceſſe eſt, quod ſine liberiore ſpiritus nitro-aerii acceſſu (præſertim ſanguine effoeto jam exiſtente)

iſtente) fieri vix poteſt. Plane ut præcipuus reſpirationis uſus eſſe videatur, ut muſculorum et præcipue cordis motus inſtituatur. Et ulterius adhuc, ſi poſtquam cordis motus ob ſuppreſſam reſpirationem ceſſaverit, aer pertubulum venæ adaptatum inſuffletur, motum cordi ex poſtliminio reſtitutum videatur. Ut videatur, aerem eſſe illud, ſine quo cordis motus inſtitui plane nequit, neque multum referre, quâ ratione, utrum per pulmones, an aliâ quâvis viâ aer in ſanguinis maſſam trajiciatur. P. 306-7.

(H) Quocirca, cum munera arteriarum umbilicalium, quæ hactenus aſſignata ſunt, minus propria, veraque videantur, liceat, cum divino ſene Hippocrate, ſtatuere, umbilicum embryo reſpirationis vicem ſupplere; quæ etiam doctiſſimi Everardi ſententia eſt. P. 318. Statuimus, ſanguinem embryi per arterias umbilicales ad placentam ſive carunculas uterinas delatum, non tantum ſuccum nutritium ſed una cum eodem particularum nitro-aerearum portiunculam commeatu ſuo ad fœtum devehere; plane ut ſanguis infantuli, per circulationem ſuam in vaſis umbilicalibus factam, eodem modo ac idem in vaſis pulmonalibus, particulis nitro-aereis impregnari videatur. Proinde, ut placenta non amplius jecur, ſed potius pulmo-uterinus, nuncupanda ſit: P. 319-21.

(I) Enimvero veriſimile eſt, ſi ſanguis arterioſus, qui ſpiritu nitro-aereo imbutus eſt, loco venoſi ad cor accederet, nullâ omnino reſpiratione opus eſt. P. 321.

(K) Et hoc inde confirmari videtur, quod dum ſanguis arterioſus ex uno cane in alterum, noto jam experimento, tranſmittitur, canis in quem ſanguis transfertur, quanquam antea anhelus et intenſe reſpirans, ſanguine tamen arterioſo intus recepto vix omnino reſpirare videtur. P. 321.

(L) Veriſimile etiam eſſe, puriorem, ſubtiliorem, magiſque penetrabilem aeris partem, dum ille inſpirando pulmonibus hauritur, ibidem quaſi deperdi, adeoque aerem expiratum, utpote fuliginibus et vaporibus implicitum iniquinatum, orbatumque potiori parte ſua, vapidum quodammodo atque eſſœtum reddi; quinetiam (ut facile crediderim) eidem elaſticitatis ſuæ vim magna ex parte perire, nempe mobiliori illa materia plurimum ſpoliato. P. 34-5*.

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* De Reſpirationis Uſu primario Diatriba. Auctore Malachia Thruſton, M. D. Lugd. Batav. 1671.

(M) Porro particulæ aeris ſanguini commixtæ, non ad fluiditatem ſolum, verum etiam ad colorem ejus conſervandum non parum valent. P. 52.

(N) Tandem hoc pulmoni proprium ac peculiare eſſe volumus. Quod intromiſſis particulis aeris, colore coccineo, rutiloque ſanguinem donet; unde non floridior tantum, ſed etiam fluidior ut ſit, neceſſe eſt; quo autem fluidior eſt, eo facilius circuitu etiam ſuo defungitur. Celeberrimus Hookius et Lowerus, jam aliquoties laudatus, nobiliſſimo experimento, rem hanc difficillimam confeciſſe videntur, nec ſinunt nos diutius de eadem dubitare. Ego vero non parum lætor, illiſque gratias ago quod theoriæ meæ (a nonnullis publice ſed ſuppreſſo meo nomine impugnatæ) rem ipſam teſtem dederint. P. 128-9.

(O) Hoc præmiſſo, non opus erit ut quæramus, cur ſtrangulati tam ſubito morte pereant, ſive illi ſuſpendantur laqueo, ſive aquâ ſubmergantur, ſive catarrhis ſuffocentur? repetendum enim eſt, aeris uſu interdictis ſanguinem a motu ceſſare, proindeque et vitam tolli. Atque hanc rationem veram eſſe, multæ variæque obſervationes, et experimenta, teſtantur. Harveius (in Epiſt. ad Riolanum) ſe in cadavere humano, noviter ſtrangulato, auriculam cordis dextram et pulmones ſanguine plurimum diſtentos, atque infarctos reperiſſe teſtatur. Nempe ceſſante reſpiratione, nihil erat, quod ſanguinem e pulmonibus in ſiniſtrum cordis propelleret, nihil quod fluidum ſatis, calidumque, ac ſpirituoſum, adeoque rite commixtum et rutilum præſtaret. P. 61-2.

(P) Nimirum inſpiratus aer et ſanguini commiſcebatur, ejuſque fluiditatem promovebat, ac totum ejus flumen, quod jam in dextro cordis ventriculo, atque in pulmonibus ſtagnarat, forti impulſu ad progreſſum et circulationem ſollicitabat. P. 63-4.

(Q) At in fœtu nihil aeris admiſceri poteſt, cum in utero non reſpiret; et tamen in eo, non minus bonus ac ſpirituoſus ſanguis conficitur, in quam homine nato et reſpirante. P. 455*.

(R) Quinimo nec a ſiniſtro cordis ventriculo, novum hunc ruborem ſanguini impertiri, certiſſimo hoc experimento confici poteſt; ſi nimirum aſpera arteria in collo nudata diſcindatur, et immiſſo ſubere arctè deſuper ligetur, ne quid aeris in pulmones ingrediatur, ſanguis ex arteria cervicali ſimul diſciſsâ effluens (ſaltem qui aliquandiu poſt præfocatum pulmonem erumpit) totus venoſus pariter, et atri coloris apparebit, non aliter quam ſi vena jugulari pertuſa profuſus fuiſſet. Hoc ego ſæpius expertus ſum. P. 119*.

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* Iſbrandi De Diemerbroeck Medicinæ et Anatomes Profeſſoris Anatome Corporis Humani. Genevæ. 1679.

(S) Itaque propulſo ſanguine atque inſufflatis ſimul, necnon perforatis pulmonibus; expectationi eventus optime reſpondebat; quippe æque purpureus in patinam excipiebatur, ac ſi ex arteria viventis effuſus fuiſſet. P. 119.

(T) Præterea colorem hunc rutilum particulis aeris ſeſe in ſanguinem inſinuantibus omnino deberi, ex eo ſatis perſpicuum eſt; quod ſicut in pulmonibus per totum floridus redditur, quia in illis aer per omnes ſanguinis particulas diffuſus, cum ipſo intimius permiſcetur, ita ſanguinis venoſi in vaſe excepti ſuperficies, et pars ſumma, quatenus aeri expoſita ſit, coccineum quoque colorem acquirit; quæ ſi cultello auferatur, proxima quæ ſubjacet a ſimili aeris contactu, in eundem brevi mutabitur. P. 121.

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* Tractatus de Corde, item de Motu et Colore Sanguinis et Chyli in eum tranſitu. Authore Ricardo Lower, M. D. Coll. Med. Lond. Socio. Editio quarta. Londini, 1680.

(v) Quare ſanguinem in ſuo per pulmones tranſitu aerem haurire, ejuſque admixtioni floridum ſuum colorem omnino debere, maxime veriſimile eſt; poſtquam autem in habitu corporis et viſcerum parenchymatis aer rurſus a ſanguine magnâ ex parte avolavit, atque per poros corporis tranſpiravit, ſanguinem venoſum illo privatum obſcuriorem et nigriorem illico apparere, rationi pariter conſentaneum eſt. P. 122.

(w) Ex quo conjicere facilè eſt, quantum ſanguini beneficium ab admixto aere accedat, quantumque interſit, eum ſalubrem ſemper et ſerenum haurire; quantumque aberrent illi, qui aeris hoc cum ſanguine commercium omnino intercedant; abſque quo fieret, ut poſſet aliquis non minus ſalubriter verſari in fætore carceris, quam inter ameniſſima vireta; ubicumque ſc. ignis ſat commode ardere poteſt, ibi et nos æque commode reſpirare. P. 123.

SECT.

SECT. VIII.

MAYOW'S *Theory of* RESPIRATION *adopted by* WOLFERSTAN *and* VERHEYEN.

When the myſtery of reſpiration began to be unfolded by men of abilities, the truths which every where adorn the works of Mayow, ſoon caught the attention. Obſervations were made, which the neglected experiments of Mayow had already confirmed, and opinions ſtarted, which his doctrine had long before beautifully illuſtrated. The ſtudy of air, too, became a favourite purſuit, and branched out into multiplied experiments, in order to account for the various phænomena, which accompany the important function of reſpiration. As Mayow, from his laborious and ingenious experiments, had explained this ſubject in the moſt accurate and ſatisfactory manner, it was natural, when truth aſſerted her dominion, to bring forward him whoſe labours moſt certainly entitled him to credit and reſpect. In a book which we accidentally met with, the author adopts the theory of Mayow, and explains himſelf, very clearly, on the ſubject*. He rejects, as abſurd, the idea of fermentation in the heart; and ſays, that the chyle coming to the right ventricle, is more intimately blended with the blood in the diaſtole; yet they ſtay not to make that buſtle ſome have thought, but by the ſyſtole, or contraction of the heart, are ſoon driven out again, the chyle accompanying the blood, to the greateſt amongſt all the the viſcera for *ſanguification, I mean the lungs*, where they both receive a *florid red*, and by the admiſſion of *new particles*, are more perfectly aſſimilated, and united with each other†: and he ſays, he very much queſtions, whether any thing elſe is left in a healthy air, to join with the blood, but the aerial ſalt.

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* An Enquiry into the Cauſes of Diſeaſes, in general, and the Diſturbances of the Humours in Man's Body, wherein the Nature of the Blood, of the Air, and of a peſtilential Conſtitution, are briefly conſidered, with ſome Obſervations on the Venom of Vipers, by Stanford Wolferſtan, M. A. London, 1692. I find no account of this author in Wood's Athenæ, neither is he mentioned by Morhof.

† P. 7.

His obſervations, on the uſe of the aerial ſalt, are very ſtriking. It is apparent, he obſerves, that the air does enter the veſiculary cells, and there, whether by adheſion or diſſolution, does depoſite, at leaſt, *ſome part* of this aerial ſalt, where meeting with the occurring humours, it is directly carried to the heart, and by its pungent particles, moleſting and irritating the ſtreight and oblique fibres of the heart, does ſtir them up into a ſyſtole, or contraction, and ſo, by ſtreightening its ventricles, to drive forth the blood contained in them, in the arteries. And *this I take to be the true original of all pulſation, and firſt motion of the heart*, and circulation of the blood throughout the body.* He ſays, he cannot ſuppoſe, that the aerial ſalt enters the pores of the body: neither, when I obſerve the difference between the florid, *aerial* coloured blood, returning from the lungs, and the atro-purpureal blood contained in the veins, can I believe this aerial ſalt does enter by the pores; or is mixed with the venal blood, when I ſee the venal blood both changed in colour and conſiſtence, from what I either find in the lungs or arteries.* He maintains, that the effects of the aerial ſalt on the ſyſtem is evident, in many caſes: it produces hunger, by entering the ſtomach and lungs; hence the appetite in taking the freſh air: that it is highly uſeful to the motion of the ſtomach:† it prevents too the coagulation of the blood, and promotes the ſecretion in our bodies, and facilitates the ſeparation and expulſion of almoſt every humour. The reader will recollect, that this was alſo the opinion of Thruſton, who maintained, that the inſpired air gave fluidity to the blood.

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* P. 24.

That oxygen renders the blood more fluid, by its union with it, we think is evident, from the following experiment, which we made. After a frog had remained ſeveral hours in nitrogen gas, he was taken out, and barely ſhewed ſigns of ſenſation, when pinched. He was now opened; the heart was turgid with dark blood, and did not act, except when pricked. In this ſtate, he was placed in a glaſs jar, containing oxygen gas. The heart began inſtantly to act, and the blood became very quickly florid, through all the veſſels. I obſerved, when opening him, that the blood in the veſſels, particularly in a large one, which runs along the internal ſurface of the abdominal muſcles, was dark, firm, and coagulated, and did not flow, but only left a ſpotted mark on the ſciſſars, where they divided the veſſel; as ſoon as it became florid, from the admiſſion of oxygen, it flowed freely through all the veſſels, and in conſiderable quantity, giving a bright red colour to the ſurface of the water.* It is far from us to build theory or practice upon diſtorted facts;

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* P. 28.

† My very worthy and ingenious friend, Dr. Ingen-Houſz, found by his eudiometer, that there was leſs oxygen in the marſhy lands of Holland, than at Vienna; and he attributes the *keenneſs* of *appetite*, and *quickneſs* of *digeſtion*, in the latter place, to this circumſtance. Mayow alſo maintains, that the fire-air particles are eſſential to digeſtion.

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* Experiments ſimilar to this, the Royal Society have done us the honour to receive. We have there proved, that oxygen is not the principle of, but ſtimulus to irritability, contrary to the opinion of Girtanner and others.

ſuch is the experiment, and the reader is at liberty, to draw what concluſions he pleaſes from it. Theſe ſuggeſtions, however, of Thruſton, and Wolferſtan, confirmed by our experiment, hold out the hopes of reſolving tumours, by the inhalation of oxygen, and accordingly this idea has been put to the teſt of experiment; and the tumours I have ſeen reduced by this remedy, affords the pleaſing conſolation, that when obſervations ſhall have been matured by further experimental enquiry, caſes of this kind will be as little dreaded by the practitioner, as the moſt trifling: and it gives me pleaſure, that the knowledge, on this ſubject, daily increaſes; for among the many caſes which come under the care of Dr. Thornton, he often meets with glandular ſwellings, particularly about the neck, which yield ſurpriſingly to the inhalation of vital air.

After having enumerated theſe effects of the aerial ſalt, Wolferſtan forms a theory, which few will imagine was entertained by ſo old a writer. He ſays, that the plague, and peſtilential fevers, muſt ariſe from either a partial defect, or partial contamination, or depravation of this ſalt. When reaſoning on the partial want of the nitro-aerial particles, he adds the following remarkable obſervations; Yet even in theſe partial defects, when only one part of one lobe of the lungs is ulcerated, yet in how ſhort a time is the motion, colour, and conſiſtency of the blood, changed from what it was? *

In caſes of peſtilential fevers, for the want of this combining ſalt, the particles of our blood are diſlocated, its craſis made lax, and prepared very ready to receive and combine with this aerial depravation†. The reader will, probably, here recollect, the reaſoning that has been applied, in the cure of typhus, by oxygen. The analogy pointed out between the blood, in the ſecondary ſtage of fever, and ſea ſcurvy, is very ſtriking. The ſame idea preſented itſelf to the penetrating mind of

Wolferſtan,

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* P. 45-6.

† P. 48. Wolferſtan ſeems to think, that only a *certain* quantity of nitro-aerial particles preſerves the proper, or if I may ſay, the vital conſiſtence of the blood. That different ſtates of aggregation, in the component parts of the blood, takes place, to a certain extent, during life, I believe is allowed; but it would be deſirable to know, how far oxygen is concerned in ſuch alterations.

Wolferſtan, when he obſerved, that the craſis of the blood was lax, for the want of the combining ſalt. Some of the moſt curious paſſages, in this valuable little treatiſe, relate, if I may apply the expreſſion to ſo early an author, to the de-oxygenation of the blood. He applies Mayow's theory, to account for the effects produced by the bite of the adder. He obſerves, that the different parts of adders produced no bad effects, when eaten by dogs, cats, &c. but when a living creature was bit, the place always ſwelled, looked black, and the blood ſeemed to coagulate.* Not being able to find, by tedious diſſection, a ſtructure for the ſecretion of poiſon, nor the poiſon itſelf; but above all, he ſays, having obſerved, that during biting, the adder kept its jaws ſo exactly cloſe, that no breath or water could paſs out, and that the animal, at the time, made an expiration, and that no ill effects were produced from the bite, if he prevented the animal from making an expiration; and after having quoted Mayow, to ſhew, that expired air is no longer fit for animal life, he concludes, that it is *the introduction of this unwholeſome breath into the ſyſtem, which produces the conſequent evils.** The ingenious Wolferſtan, draws another concluſion from theſe facts, to explain the ſymptoms attending peſtilential fevers. 'Tis as eaſy to conceive, he adds, as in the adder, how the ſame deficiency is made in the air we breathe in, and what effect that deficiency will have upon the humours of our body.†

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* P. 62. Baglivi gives a ſimilar account of the bite of the tarantula. Demorſa pars quandoque ſtupore inſigni prehenditur *circulo livido, nigro,* aut *ſubflavo ſtatim* coloratur. And an obſervation which he adds is ſo remarkable and curious, that I muſt quote it. Generaliter loquendo ſymptomata tarantalorum, primo veneni inſultu ſimilia ſunt iis, quæ in malignâ ſebre ex coagulatione obſervari ſolent, qualia ſunt angor cordis ſpirandi difficultas, omnimodo fere pulſus oppreſſio, animalium vitaliumque actionum repentina, ac pene fatalis jactura. Diſſert. de Anat. &c. et Tarant. P. 23-5. How ſimilar this, to the deſcription of ſcurvy!

It is ſuppoſed now, by ſome, that various poiſons act by de-oxygenating the ſyſtem. May not many animals poiſon, by introducing into the ſyſtem, expired air? It is known, from experi-

ments,

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* P. 70. † P. 74.

ments, that carbonic acid and azotic air, injected into the blood veſſels of animals, produce a dark colour of the blood, and ſpeedily deſtroy life.* It is found, alſo, that the preſence of the deleterious gaſes, to any conſiderable extent, more or leſs prevents the union of oxygen with different ſubſtances. Hence, probably, the reaſon, why ſo ſmall a quantity, introduced into the circulating ſyſtem, produces death. The celebrated Italian experimentaliſt, Fontana, has ſhewn, that the venom of the viper, mixed with the blood of a fowl, rendered it black, and preſerved it in that ſtate, notwithſtanding its expoſition to atmoſpheric air; and we know, that under this latter circumſtance, its ſurface ſhould have acquired a florid colour. Hence the venomous influence, prevented the abſorption and uſual agency of oxygen.

The doctrines of Mayow, thus elucidated, in ſome degree, by Wolferſtan, began now to make their way on the Continent, and to acquire further able ſupporters. The clearneſs and preciſion, with which they pointed out the neceſſity of the preſence of an aerial fluid, to the vitality of the blood, demonſtrated, that reſpiration was not intended for thoſe futile purpoſes, to which it had been hitherto applied. Some of the beſt ideas on this ſubject, after Mayow, were thoſe publiſhed by Verheyen. We cannot avoid mentioning ſome remarkable obſervations of this author, as they clearly ſhew, that he perfectly underſtood this important function; particularly, too, as he has attended, with more accuracy, than any one, to Mayow's experiments, and has altogether adopted the reaſoning and doctrine founded upon them. He rejects, with Mayow, that reſpiration is to cool the blood, or to tranſmit it through the lungs; other more important uſes muſt be ſought for. (A) From conſidering, with attention and accuracy, every circumſtance, it appears, that reſpiration is performed, for the expreſs purpoſe of continually ſupplying air to the ſyſtem, to keep up the motion of the blood; for it is evident, that our life and health altogether depend upon this cauſe. Now, when we obſerve, that this motion fails, when reſpiration is impeded, it is rational to conclude, that ſomething is ſupplied from the air, through the medium of reſpiration; more eſpecially, too, when we recollect, that the blood acquires its beautiful crimſon colour in the lungs. The blood, however, he remarked, loſt this colour, in its circulation through the ſyſtem, for it is obſerved to be dark in the veins. (B) Therefore, ſince the veins receive the blood from the arteries, it follows, that its bright red colour is loſt, either in the capillary branches, or in its paſſage from the arteries to the veins; and ſince the bright crimſon colour is reſtored to the blood, after it has circulated through each ventricle, and the intermediate lungs, we muſt conclude, that the colour originally loſt, is reſtored to it, during its paſſage from one ventricle to the other. Here the experiments of Lower are quoted, in confirmation of his opinion, and he makes the following curious remarks. (C) The laſt portion of blood drawn, during venæſection, has very frequently the appearance of being arterial. It is owing to this cauſe; as the blood in the veins flows ſlowly, it oppoſes ſome reſiſtance to the circulation of the arterial blood, which, when moderate, is of uſe, for the ſafe and eaſy ſecretion of various principles, from the maſs of blood; but during the flow of blood from an opened vein, that reſiſtance being removed, the arterial blood circulates more freely, and therefore does not depoſit, in ſuch quantity, thoſe principles, which it otherwiſe would have done, and *among which we muſt take in the matter ſupplied by the air.*

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* Girtanner.

Verheyen was not ſatisfied with aſſerting, that air entered the blood, during reſpiration; like Mayow, he muſt enqire what that aerial matter is, which is ſo neceſſary to liſe. Here he quotes this author, and adopts his opinion, that the nitro-aerial particles is the matter ſupplied. He agrees with him, too, in thinking, that that part of the air is copiouſly condenſed in nitre; hence it is, that combuſtible matter readily burns in vacuo, when combined with that ſalt. (D) I could adduce, adds Verheyen, many more arguments, drawn both from reaſon and experience, in favor of the opinion, that ſuch an aerial ſupply is acquired by reſpiration; but the want of time, and a wiſh to conſult the convenience of the reader, prevent me: if any one, therefore, is deſirous of more knowledge, on this ſubject, he may conſult the treatiſe, which the ingenious Mayow has publiſhed, on nitre and the nitro-aerial ſpirit. Verheyen goes on to prove, that the nitro-aerial particles are taken into the ſyſtem, for the purpoſe of producing heat and motion, which are cauſed by the mutual union of the nitro-aerial and ſulphureous particles. We now know the reaſon, he ſays, why reſpiration is ſo much quickened by exerciſe, and why animals die in the vacuum of an air pump; for together with the air, are exhauſted thoſe particles ſo neceſſary to life. (E) Indeed it is very probable, that together with the air, are included other particles, of a more ſubtile nature, differing very much in their nature and properties. And it is certain, that animals die in vacuo, on that account, becauſe the flame and ignition of coal, a candle, and ſuch like ſubſtances, confined in the glaſs, are immediately extinguiſhed, on the exhauſtion of the air, if we are to credit the experiments of Boyle; but gun-powder, which beſides ſulphureous, contains nitrous matter, burns and flames almoſt equally as well as in the open air.

This author, by attending to the experiments of Boyle and Mayow, acquired a perfect knowledge of the function of reſpiration; and his ideas on the motion and heat of the blood, are no leſs accurate. Theſe effects, he ſays, are produced, by the union of the particles, taken in by the lungs, with the phlogiſtic matter, which is conveyed to the ſyſtem with our food.

SECT. IX.

MAYOW'S APPLICATION *of* FIRE-AIR *to* VEGETATION.

The new diſcoveries in chemiſtry, have thrown as much light on the œconomy of plants, as on that of animals. It is now evident, that the food of vegetables is chiefly air. The ingenious Prieſtley, the world well knows, inſtituted a variety of experiments, to diſcover what air was beſt adapted to the food of plants; and at laſt determined, that plants grew the moſt vigorouſly, in noxious and putrid airs. All the noxious airs, however, were not equally favourable to vegetation; nitrous air, he obſerved, preſently deſtroyed it. The experiments of Prieſtley diſcover much contradiction; in ſome caſes, the plants thrived perfectly well in putrid air, but in others they died.* Dr. Prieſtley alſo found, that plants have a power of correcting bad air; but did not know the condition, neceſſary to make plants perform ſuch an office; for both he and Mr. Scheele obſerved, that the growth of plants very frequently injured the air. Theſe contradictions could not be accounted for, by the diligent enquiries either of Prieſtley, of Senebier, or of Scheele, till my accurate and learned friend, Dr. Ingen-Houſz, clearly pointed out the error. He diſcovered, that not only the ſame plant did not, at all times, give out the ſame kind of air, but alſo that different parts of the ſame plants, afforded different kinds of air; and that theſe variations were owing to the influence of the ſun's light upon vegetation. His experiments too, led him to draw very different concluſions from thoſe of Prieſtley. This philoſopher imagined, from his experiments, that oxygen was invariably injurious to vegetable life. This opinion, however, Dr. Ingen-Houſz has refuted, and maintains, that plants ſhut up in vital air, live ſo much the longer, as the air is ſuperior, in purity, to atmoſtpheric air.* The uſe of oxygen is, to form with carbon, fixed air, the chief food of plants. Being perſuaded of this, from his experiments, he concluded, that the ſoil was continually engaged, in forming carbonic acid, for the nouriſhment of vegetables; and that, for this purpoſe, it muſt abſorb oxygen from the atmoſphere, being already ſupplied, by a variety of ſubſtances, with carbon: hence the neceſſity of fallow. To prove this, he made the following experiments; He expoſed eight cubic inches of good mouldy ground, to eighteen of atmoſpheric air, for three days, but conſtantly hid from the ſun's light, by covering the apparatus with a flower-pot, and found the air ſo much deprived of oxygen, that a wax taper could not burn well in it; and when the experiment was made in the light, the remaining air was almoſt pure azot.*

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* Vide Vol. I. for 1781, p. 86.

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* Eſſay on the Food of Plants, p. 2. Is it not probable, that the food of different plants vary, like that of animals?

This,

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animals? Thoſe which grow in marſhy grounds, it is probable, have their food principally compoſed of hydro-carbonate, which is diſengaged, in conſiderable quantity, in ſuch ſituations. Dr. Prieſtley found, that the willow thrived the beſt of all in putrid air; and this plant, we know, conſtantly grows in marſhy ſoils.

* Ibid. p. 16-17.

This, ſurely, has let in conſiderable light upon agriculture, and ſhews the great influence of oxygen, in the wide extent of the creation. This uſe of fire-air, did not eſcape the penetration of the ingenious Mayow. We have ſeen, how admirably he has applied this air, to the purpoſes of the animal body; his explanation of the vegetable proceſs, upon the ſame principle, is no leſs curious and remarkable. He ſuppoſed, however, that nitre formed the chief nouriſhment of vegetables; but the manner in which he has explained it, diſcovers how accurately he marked the agency of oxygen, in the vegetable kingdom. It was his opinion, that the fire-air particles united with various ſubſtances in the ſoil, but chiefly with the alkaline baſe of nitre.

(F) The nitro-aerial ſpirit ſlowly and gradually effervеſcing with the earthy matter, varies its ſulphureous part to a proper volatility; and moreover unites with the ſaline part into nitre; the elementary principles are formed in ſuch a manner, as to promote the growth of vegetables. It will be evident, that this is the caſe, if we conſider at what ſeaſon of the year, chiefly, the nitro-aerial ſpirit effervеſces with the earthy matter, and forms nitre. Now, we know, that nitre is produced, in greater quantity, in the ſpring, than at any other time: for, in the winter, the nitro-aerial particles and earthy ſulphur, are bound by the froſt; but on the approach of ſpring, the nitro-aerial ſpirit is brought into motion, by the warmth of the ſun, and the ſoil is opened, the froſty weather diſappearing: hence the nitro-aerial ſpirit, being now more active, deſcends deep into the earth, and there meeting with ſalino-ſulphureous particles, produces a violent effervеſcence, whence nitre is copiouſly generated, and vegetation comes on rapidly.* That oxygen unites with other matters beſides carbon, to form the food of plants, is evident, from a variety of circumſtances. The many heterogenious ſubſtances, which enter into the compoſition of common mould, muſt contain various elementary principles, ready to unite with oxygen, in various propotions, according to the degree of temperature. It is needleſs to particularize any one baſe, with which it will unite, as the reader can ſupply many from his own obſervation. It is evident, that it will form acids, and then neutralize ſalts, *ad infinitum*, according to the ſubſtances it meets with: and in a letter to Dr. Ingen-Houſz, we have mentioned it as probable, that the great quantity of oxygen abſorbed by the ſoil, contributes to form thoſe vaſt reſervoirs of water, found in the bowels of the earth.

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* Vide Appendix, No. IV. and V. where Dr. Ingen-Houſz has given a full account of his diſcovery, that plants give out oxygen, by the influence of the ſun's light, and where the coincidence between his opinion and that of Mayow is further mentioned.

The neceſſity of air, then, to the growth of plants, has been fully proved, by Hales, Prieſtley, Ingen-Houſz, and Scheele. A plant ſhut *in vacuo*, ſoon dies, ſays Dr. Ingen-Houſz; and it is probable, obſerves Hales, that the air freely enters plants, not only with the principle fund of nouriſhment by the roots, but alſo through the ſurface of their trunks and leaves, eſpecially at night, when they are changed from a perſpiring to a ſtrongly imbibing ſtate.* Dr. Ingen-Houſz, too, has not only proved, that plants can exiſt altogether upon air, by being hung up to the ceiling of a hot-houſe, but has alſo ſhewn, as we have ſeen, that the aerial fluid is ſo neceſſary to their exiſtence, that it is abſorbed in great quantities by the ſoil, for the purpoſes of nouriſhment. It would appear, from the poſitive manner in which Mayow expreſſes himſelf, that he had made experiments to prove the very ſame thing. (G) So neceſſary indeed, he obſerves, is this aerial ſalt to every kind of life, that not even plants can vegetate in earth, from which the acceſs of air is precluded; but if this earth be expoſed to the air, it becomes impregnated with this fœcundating ſalt, and is again rendered fit for the nouriſhment of vegetables: which proves, that the very plants have a kind of reſpiration, and are under the neceſſity of imbibing the air.

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* Vegetable Statics, Vol. I. p. 59. The abſorption of air, by plants, was proved by a very beautiful experiment. P. 155. Expert. 47.

(A) Itaque omnia rem iſtam concernentia exacte perpendens, exiſtimo, reſpirationem eo potiſſimum collimare, ut quædam materia ſanguini, pro motus illius continuatione jugiter ex aere ſub-

miniſtretur; enimvero evidenter conſtat, vitam noſtram, adeoque et ſanitatem, motu ſanguinis omnino dependere. Cumque videamus hunc motum, ob defectum reſpirationis ſtatim deficere, non inepte concludimus, ejuſmodi materiam ex aere, mediante reſpiratione, ſuppeditari, maxime ſi attendamus, ſanguinem ſplendorem ſuum, et intenſum ruborem, præcipue nanciſci in pulmonibus. Vol. II. P. 326*.

(B) Itaque cum venæ recipiant ſanguinem ex arteriis, ſequitur, colorem ejus intenſe rubrum deperdi, vel in ramis capillaribus, vel in tranſitu ex arteriis in venas; et cum idem ille ſanguis fuſcus, poſtquam utrumque cordis ventriculum et pulmones intermedios peragravit, redeat totus rutilans et coccineus, conſequens eſt, colorem illum ante deperditum ipſi in tranſitu eſſe reſtitutum. P. 327.

(C) Pro quo notandum, quod ſanguis in venis tarde fluens, aliquatenus reſiſtat tranſitui ſanguinis adventantis per arterias, quæ reſiſtentia

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* Corporis Humani Anatomia, autore Philippo Verheyen, in Univerſitate Lovanienſi Art. et Med. Doct. et Profeſſore. Amſtelodami et Lipſiæ. 1731.

moderata, utilis eſt, ut ea, quæ a ſanguinis maſſa ſeparari debent, facilius feliciuſque ſeparentur: at vero dum aperta vena ſanguis effluit, ſublata dicta reſiſtentia, ſanguis arterioſus liberius ſubſequitur, adeoque minus deponit illa, quæ alias fuiſſent deponenda, inter quæ eſt ipſa materia ex aere deſumpta P. 329.

(D) Poſſum, pro eadem reſpirationis materia, ejuſque neceſſitate, afferre plura argumenta, tam ab experientia quam a ratione petita; ſed temporis anguſtiæ et lectoris commoditati conſulendum duxi: ſi quis ulteriora deſideret, legere poterit tractatum, quem prælaudatus D. Mayow, de ſale nitro et ſpiritu nitro-aereo, conſcripſit. P. 331-2.

(E) Etenim maxime veriſimile eſt, inter particulas aere incluſas, alias aliis eſſe ſubtiliores, atque inter ſe natura et indole omnino diverſas. Et certe animalia in loco aerem non continente, ob eam rationem interire, inde confirmatur, quod ignis carbonum, candelæ, et ſimilium, magno vitro incluſus (ſi experimentis Boyleanis fidendum eſt) extracto aere confeſtim extinguatur: pulvis tamen pyreus, qui præter ſulphur materia nitroſa abundat, fere æque ac in libero aere accendatur, ardeatque. P. 339.

Spiritus

(F) Spiritus nitro-aereus, cum materia terreſtri motu obſcuro effervefcens, partem ejus ſulphuream ad juſtam volatilitatem perducit; et inſuper cum parte ejus ſalina in nitrum coaleſcit, principia verum naturalium in tali conſtituuntur, qualis ad vegetabilia formanda requiritur. P. 52. Rem modo prædicto, in vegetabilium ortu ſe habere, indicio eſt: quod quo tempore vegetabilia præcipue a terrâ oriuntur, ſpiritus nitro-aereus cum materia terreſtri maxime exæſtuat, et ſalnitrum præcipue in terra generatur: quippe experientia conſtat, nitrum majori copia ineunte vere, quam reliquis anni temporibus in terra naſci. Nimirum tempore hyberno, particulæ nitro-aereæ et ſulphur terreſtre glacie conſtipantur; ineunte autem vere, ſpiritus nitro-aereus, æſtu ſolis intenſiori in motum concitatur, terræque compages, glacie jam liquefacto reſeratur: hinc ſpiritus nitro-aereus, in motu poſitus, in altam tellurem deſcendit, ibidemque cum particulis ejus ſalino-ſulphureis minutim perfractis congreſſus, effervefcentiam ſatis intenſam efficit; unde nitrum copioſe generatur, et vegetabilia conſertim excreſcunt. P. 52-3.

(G) Adeo enim ad vitam quamcunque ſal iſtoc aereum neceſſarium eſt, ut ne plantæ quidem in terra, ad quam aeri acceſſus præcluditur, vegetari poſſint; ſin autem terra iſta aeri expoſita, ſale hoc fæcundante denuo impregnetur, ea demum plantis alendis iterum idonea evadet. Plane ut vel ipſæ plantæ aliqualem reſpirationem, aeriſque hauriendi neceſſitatem habere, videantur. P. 52-3.

SECT. X.

Of MUSCULAR MOTION.

The beautiful diſcoveries of the ingenious Mayow, enabled him to extend his theories and obſervations, to ſeveral functions of the animal œconomy. To a man who contemplates, with ſerious attention, the ſtructure of the human frame, the cauſe of muſcular motion muſt preſent many perplexing ideas. A ſelf-moving animal, is a machine of wonder to the eye of the beholder; and nothing but cuſtom, has familiarized our minds to ſuch objects. That an animal ſhould become loco-motive by volition, or a ſimple exertion of will, has afforded matter for ſpeculation and diſpute, in almoſt every age.* The laborious anatomiſt

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* Does there appear any principle, in all nature, more myſterious, than the union of ſoul and body, by which the ſpiritual part poſſeſſes ſuch an influence over the material, that it is able to direct the motion of any, or even ſometimes a part of a muſcle? *Hume.*

anatomiſt could not diſcover the cauſe in tedious diſſection; and it was equally concealed from the reaſoning of the ſpeculative theoriſt. It was little imagined, that reſpiration had any connection with muſcular motion, or that a conſtituent part of the atmoſphere, at all aſſiſted in this extraordinary function. When the mind was fatigued with reaſoning, and nothing ſatisfactory appeared, it is curious to obſerve, that it was attributed to a *cauſe*, originally impreſt upon the works of the creation. As it has been proved, that all matter is in a ſtate of perpetual motion, originally impreſt upon it by nature, it ſeems moſt agreeable to the analogy of nature, to refer muſcular motion to an original law of animated matter, for which no cauſe can be aſſigned, any more than for gravitation, coheſion, or chemical affinity.* It is with diffidence, we diſſent from ſo high an authority as that of Dr. Blane. The univerſality of motion is evident to all; but ſuch an explanation of muſcular action, is equally applicable to dead as to living matter. The Deity has, no doubt, impreſſed certain determinate and immutable laws on inanimate matter; but if that were the caſe with the animated creation, to what purpoſe has he given the living principle? That the muſcular fibre is every way fitted for that motion, to which it is applied, is a queſtion which no one can deny; but that the cauſe of this motion is ſimilar to that of gravitation, coheſion, or chemical affinity, is a poſition, which an acquaintance with the animal œconomy entirely rejects, as unfounded.

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* A Lecture on Muſcular Motion, read at the Royal Society, by Gilbert Blane, M. D. F.R.S.

The difficulty of this ſubject, did not deter the boldneſs of Mayow's genius to attempt an explanation. When we conſider, that his nitro-aerial particles are the ſame with oxygen, when we reflect, that his ideas of reſpiration, and its application to the purpoſes of the œconomy, exactly coincide with thoſe of modern times, we may expect to find his ſpeculations on muſcular motion, to form a ſtriking analogy with thoſe of ſome recent authors. When the nerves were found to have ſo great a ſhare in muſcular motion, it was a natural concluſion, that what was denominated animal ſpirits, ſhould be called in to aſſiſt, in the explanation of the motion of animals. A difficulty,

however,

however, ſtill preſented itſelf, which required conſiderable labour and inveſtigation to ſurmount; this was the nature of the nerves themſelves, a ſubject which always occaſioned much perplexity, and is not to be developed or unravelled by ſpeculations alone. Of late it has been imagined, that the diſcovery of animal electricity, opened a wide field, to acquire a *certain* knowledge of the cauſe of muſcular contraction. An ingenious author, on this ſubject, takes conſiderable pains to prove, how fully he is convinced, that the nervous electricity is the animal ſpirits, and that to the influence of this cauſe, is owing the contractile power of the fibre. It is remarkable, that the ſame reaſoning which Mayow applied to his nitro-aerial particles, has been called in to aid the explanation of the nervous electricity, by Valli. It is time, that we ſhould now examine what Mayow has ſaid, upon this ſubject, and we ſhall be as conciſe as poſſible.

(A) I maintain, that muſcular contraction is produced by different particles, uniting and efferveſcing, in the ſtructure of a muſcle. We cannot doubt, but that an influx of animal ſpirits is neceſſary, becauſe if a nerve be cut or obſtructed, the muſcle to which the nerve is diſtributed cannot contract. But we are not to imagine, that muſcular contraction ariſes from the animal ſpirits alone; in as much as to the performance of which, it is abſolutely neceſſary, that other particles ſhould be ſupplied from the maſs of blood. Since, indeed, the arterial blood flows to the muſcles, in a continual ſtream, and that in a greater quantity than is diſtributed to other parts, or than is ſufficient for their nutrition, eſpecially in violent actions, it is rational to conclude, that arterial blood, during its paſſage, depoſits ſomething neceſſary to muſcular contraction. To prove this, he relates an experiment of Steno's, who, by tying up an artery, found, that the muſcle to which it was diſtributed, was unable to undergo contraction*. (B) As to the nature of the *motive* particles, ſecreted from the maſs of blood, we are of opinion, that they are of a ſalino-ſulphureous nature. For we muſt obſerve, that not a ſmall loſs of fat takes place, in violent exerciſe, and it is almoſt altogether conſumed, in laborious employments long continued: but, on the contrary, animals indulging in eaſe, and ceſſation from labour, become corpulent, and fat is depoſited, in conſiderable quantity, upon the muſcles; whence we conclude, that the ſulphureous particles of the blood, claim ſome ſhare in performing muſcular contraction.

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* This aſſertion, ſays Valli, has been proved to be void of foundation, by the obſervations of Kaaw, Boerhaave, Langruiſh, Pozzi, and ſeveral others.

Having thus explained one ſet of particles, neceſſary to muſcular action; he next enquires into the nature of others, *viz.* animal ſpirits, which are alſo neceſſary to this function. Concerning the nature of animal ſpirits, we may conclude, that they are formed from the nitro-aerial particles. To underſtand this, he ſays, we muſt recollect, that the uſe of reſpiration is, to tranſmit nitro-aerial particles into the blood, and alſo, that they are neceſſary to motion. (c) Hence animals, in the more violent exerciſes, as in ſwift running, are obliged greatly to quicken their reſpiration, which ſeems to take place, becauſe the nitro-aerial particles are ſecreted from the maſs of blood, and conſumed during muſcular contraction.

tion.* For I think, that the nitro-aerial particles, coming from the brain into the moving parts, and there meeting with the ſalino-ſulphureous particles, they efferveſce, and by theſe being thrown into agitation, muſcular motion is performed.† Hence it is abſolutely neceſſary, for the continuation of animal motion, that a ſupply of ſalino-ſulphureous, and nitro-aerial particles, ſhould never be wanting in the maſs of blood; and in proportion as muſcular contraction is increaſed, as in the more violent exertions, ſo is there a greater waſte of nitro-aerial and ſulphureous particles; to reſtore which, not only reſpiration muſt be quickened, but alſo aliment, replete with ſalino-ſulphureous particles, muſt be taken in greater quantity: hence thoſe ſubſtances, which conſiſt principally of a volatile ſalt and ſulphur, are the moſt proper for recruiting the ſtrength, worn out by continual labour. Whence are the nitro-aerial particles ſupplied in muſcular motion? is a queſtion, which Mayow aſks himſelf. (D) Concerning this, I was ſometime in doubt, whether the nitro-aerial particles did not immediately paſs from the blood, into the moving parts; but when I had applied more attention to the ſubject, it appeared more probable, that the *motive* particles, ſupplied by the blood, were of a ſalino-ſulphureous nature: whence it follows, that the nitro-aerial particles are derived from the brain, and, conſequently, that they are the ſame with the animal ſpirits. I think it may be allowed, too, that in the more perfect animals, the animal ſpirits are only formed in the brain, and thence diſpenſed through the ſpinal marrow, and nerves ariſing from it. Why ſhould not the animal ſpirits be rather derived from the atmoſphere, than from our food? It is apparent, that the air is impregnated with extremely active and ſubtile particles, and ſo great is the demand of the ſyſtem for air, that we cannot exiſt one moment of time without it. And indeed it does not ſeem poſſible, that the immenſe waſte of animal ſpirits, can be ſupplied from any other ſource than from the air.

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* As oxygen is neceſſary to the contraction of the muſcles, it is probable, that it is conſumed; or, more properly ſpeaking, enters into ſome new combination, in conſequence of which it is eliminated out of the body; for we cannot but ſuppoſe, that the quantity employed, correſponds to the vaſt quantity imbibed. Hence we may underſtand the final cauſe of quickened reſpiration, during great exertions of the muſcles.—Beddoes on Scurvy, &c. P. 50.

† Does not muſcular contraction, or intumeſcence, really depend upon the combination of oxygen with hydrogen and azot (ſeparately, and combined in various proportions) in conſequence of a ſort of exploſion, produced by the nervous electricity?—Beddoes' Remarks on Girtanner's Eſſay on Irritability, P. 258.

Such is the admirable manner, in which our author reaſons upon this ſubject. The attention that has been beſtowed on this department of phyſiology, points out, at once, its difficulties. The elaborate inveſtigations of Valli, of Pugh, and of Blane, with all their experience, from the time of Mayow, have ſcarcely been ſufficient to the taſk. The explanation of the mechanical philoſopher, is rejected by the chemiſt, who thinks ſomething more is neceſſary, to produce the phænomena. Had all the muſcles been ſubject to volition, leſs difficulties would have perplexed the ſubject; as it is, muſcular action depends on ſomething excluſive of that cauſe; and it is as certain, too, that the action of volition alone, is not adequate to produce the effect. The will employs its agent; and what that agent is, has been the diſpute for many ages. The æther of Sir I. Newton, the fire-air particles of Mayow, with the oxygen and nervous electricity of modern times, have perplexed, divided, and confounded the medical world. The doctrine which ſeems moſt adequate to explain the phænomena, is that founded upon the curious diſcovery of Galvani. Here too, inſurmountable difficulties attend; and notwithſtanding the ingenious and plauſible endeavours of Valli, to reduce under the principle of the electric fluid, the general effects produced by the influence of the nervous ſyſtem, there are ſtill innumerable exceptions.

Although I hold it highly injurious to ſcience, to ſay the Deity wills it, when we are unable to explain certain phænomena, it is, nevertheleſs, allowed by all, that there are certain bounds placed to our knowledge, beyond which it is not permitted us to explore; and it would be no difficult matter to prove, that were our intellectual faculties more refined or intuitive, or our ſenſibilities more

acute,

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* The aſylum ignorantiæ of Spinoza.

acute, we ſhould be very unfit inhabitants of this globe. In contemplating man, ſays an ingenious philoſopher, as at the head of thoſe animals with which we are acquainted, a thought occurred, that no ſentient being, whoſe mental powers were greatly ſuperior, could poſſibly live and be happy in this world. If ſuch a being really exiſted, his miſery would be extreme. With ſenſes more delicate and refined, with perceptions more acute and penetrating, with a taſte ſo exquiſite, that the objects around him could by no means gratify it, obliged to feed upon nouriſhment too groſs for his frame, he muſt be born only to be miſerable, and the continuation of his exiſtence would be utterly impoſſible. Even in our preſent condition, the ſameneſs and inſipidity of objects and purſuits, the futility of pleaſures, and the infinite ſources of excruciating pain, are ſupported with great difficulty, by refined and cultivated minds. Increaſe our ſenſibilities, continue the ſame objects and ſituation, and no man could bear to live.* Hence the Heathen ſuperſtition, attached ſomething præternatural to the mode of life, nature, and food of their gods, as we find in the deſcriptions of their antient philoſophers and poets. Far be it from me to damp the ardour of inveſtigation; and it is impoſſible for us to ſay, what can or cannot be known, as we have not yet arrived at, or acquired the full completion of *time;* but it appears, that muſcular motion depends upon an immaterial cauſe, little cogniſable by our ſenſes, and originally implanted in animal nature by Creative Wiſdom, to direct the inſtinct and the reaſon of the rational and irrational part of the creation.

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* Smellie's Philoſophy of Natural Hiſtory. P. 526.

SECT. XI.

Of the CONSPIRING ACTION *of the* INTERCOSTAL MUSCLES.

The accuracy of Mayow, as an anatomiſt, is no leſs conſpicuous than his ſkill as a chemiſt. His attentive obſervation of the animal œconomy, from the ſlow-paced labour of diſſection, led him, no doubt, to the knowledge of the oblique co-operation of muſcles; a diſcovery which Dr. Monro has ſo excluſively claimed to himſelf, that many will be juſtly ſurpriſed, when they read the quotations from Mayow. Firſt, we ſhall briefly lay before the reader, Dr. Monro's doctrine, with his reaſoning; and after, by bringing paſſages from Mayow, we ſhall be the better able to judge of the circumſtances. Dr. Monro, after giving an account of muſcular obliquity, in general, adds, ‘ but in treating on particular parts, I dwelt chiefly on the ſtructure and effects of the intercoſtal muſcles, as a variety of opinions, concerning their operation, has, in the courſe of the laſt hundred years, been propoſed; and as *no author had explained the reaſon of the obliquity of their fibres, nor of their being diſpoſed in two layers of decuſſating fibres*, after fully explaining the ſtructure, I endeavoured to prove, as Haller had done, but with ſome additional arguments, that both rows of intercoſtal muſcles, conſpired to elevate the ribs, or that they were muſcles of inſpiration.'* After having aſſerted this, he propoſes his reaſons and illuſtrations. That the firſt rib is ſo much fixed, as to be almoſt immoveable; and that the ſecond rib is more fixed than the third, and the third than the fourth, and ſo on, downwards.†

Now let us ſee, what Mayow has ſaid upon this ſubject. (E) It is a received opinion, that the external intercoſtal muſcles ſerve to dilate, and the internal to contract the thorax; but it appears to me more rational, that the thorax is dilated,

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* Obſervations on the Muſcles, and particularly on the Effects of their Oblique Fibres, by A. Monro, M. D. Edinburgh, 1794. P. 15.

† P. 16, 17.

lated, at the ſame time, by both. And we may aſſert, that the thorax is dilated, when the ribs are raiſed, and on the contrary, contracted when they are depreſſed. Let us ſuppoſe here (of what any one may be ſatisfied by examining the ſkeleton) that the ribs are not articulated with the ſpine and ſternum, at right angles, but that the angle below the ribs is ſomewhat leſs than a right one, ſo that if a rib be raiſed, its articulations with the ſpine and ſternum approach to right angles. Moreover we maintain, that the thorax is dilated, by the ribs being brought to right angles.* Whenever a muſcle attached to two bones contracts, the one bone approaches the more fixed; therefore, ſince each lower rib is leſs fixed than the upper one, it follows, when the intercoſtal muſcles contract, that each lower rib muſt be drawn upwards. And the ſame takes place, during the action of the

internal,

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* Haller mentions ſomething ſimilar to this. Coſtæ, circa ſuas articulationes rotatæ, angulos et cum ſterno et cum vertebris *majores* faciunt, in mediis vero arcubus aſcendunt, marginem ſuum inferiorem antrorſum erigunt. Primæ Lineæ Phyſiol. P. 162. It muſt be remembered, that Haller quotes Mayow, in his Elements of Phyſiology.

internal, as well as external intercoſtal muſcles; nor is it of any conſequence, that they are inſerted into the ribs at different points, as appears in *Fig.* 1, where the interior muſcle, *a. a.*, during its contraction, draws up the lower and more moveable rib, equally as well as the exterior muſcle, *b. b.* We muſt here obſerve, that the ribs are articulated, in ſuch a manner, with the ſpine, that when they are raiſed by the action of the intercoſtal muſcles, they eaſily aſcend, and are thrown out, ſo as to deſcribe a circle.*

Can expreſſions and reaſoning be more ſtrikingly ſimilar? In ſome places it is almoſt a literal tranſlation. Dr. Monro further obſerves, that from the origin and inſertion of the intercoſtals in an upper and lower rib, they act with a longer lever upon the upper rib;† and ſays, it is evident, that the obliquity of the fibres here, is not intended to increaſe their number or ſtrength of the muſcle; becauſe the fibres would have been more numerous, if they had paſſed directly from one rib to the other, or had been inſerted into the ribs at right angles.* Let Mayow ſpeak for himſelf. After obſerving that both ſets of intercoſtals conſpire to raiſe the ribs, he adds, (F) The oblique and oppoſite ſituation of the intercoſtal muſcles, is a farther proof of this. Although an inſertion at right angles would have been better adapted, for directly raiſing or depreſſing the ribs, yet nature ſeems to have inſerted thoſe muſcles obliquely into the ribs, becauſe the intercoſtal ſpaces are ſo ſmall, that if thoſe muſcles had been inſerted at right angles, they would have been ſhorter than their nature allows; therefore, that they might have their proper length, it is neceſſary that they ſhould be inſerted obliquely into the ribs. But ſince this oblique poſition is leſs fit for raiſing the ribs, therefore nature, that excellent mechanic, has formed muſcles of various inſertion, that, whilſt they at different points obliquely raiſed the ribs, the ribs ſhould, nevertheleſs, aſcend directly upwards, as is ſeen in *Fig.* 2, where the exterior muſcle, *a. a.*, and the interior, *c. c.*, acting at the ſame time, the inferior and more moveable rib

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* Vide the quotation from Haller, in note, P. 179.

† P. 17.

aſcends

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* P. 21.

aſcends directly upwards, in like manner as if it had been acted upon, by a muſcle inſerted at right angles. From this it is plain, that both the internal and external intercoſtal muſcles, acting at the ſame time, raiſe the ribs, by their joint co-operation, and thus dilate the thorax. Mayow further maintains, that the ribs are thrown out, during inſpiration; and obſerves, that they are united, by a double articulation, to the vertebræ. His words are, (G) It is to be remarked, that the ribs are not, as is commonly believed, united to the ſpine by a ſingle, but by a double articulation; and theſe articulations are ſo obliquely placed, and are formed with ſuch art, that it is impoſſible that the ribs ſhould be raiſed by the intercoſtal muſcles, but that they muſt, at the ſame time, be thrown out, for the more ample dilatation of the breaſt, as is evident from *Fig.* 3. In which let *a. e. i.* be a portion of a rib, whoſe round head *a.* enters the ſinus *c.* in the ſpine, which articulation is ſuperior and anterior. On the contrary, in the other articulation, *viz.* the inferior and exterior, the ſinus, which is ſmaller, is hollowed out in the rib, at *e.*, which is articulated with the ſpine, at the protuberance *b*. Now if we ſuppoſe *a.*, the head of the rib, to be placed in the ſpinal cavity *c.*, and the coſtal ſinus *e.* to reſt upon the ſpinal protuberance *b.*, and then if that rib, connected to the ſpine by theſe double articulations, be moved upwards, it is eaſy to conceive, that that rib would be thrown to the left, or which is the ſame, with reſpect to the ſituation of the thorax, would be extended outwards.* An ingenious and accurate anatomiſt of London, takes conſiderable pains to impreſs upon the minds of his pupils, the effects produced by the double articulation of the ribs. He maintains, that the ribs are, thereby, much aſſiſted, in rolling upwards and outwards. It is unneceſſary for me to mention, how far this is the opinion of Mayow.

The acute Mayow offers another reaſon for his

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* The celebrated Author of the Conſpectus Medicinæ, has the following obſervations. Hæ (ſc. coſtæ) autem, propter obliquum quem cum ſpina dorſi habent ſitum et nexum, elevari nequeunt, quin ſimul protrudantur ipſæ et ſternum quoque protrudant et latera pectoris oppoſita a ſe invicem aliquantum dimoveant. Vol. I. p. 295.

his opinion, that the ribs are thrown out, during inſpiration, and which, we think, is an irrefutable fact. (H) Nor is it to be neglected, that the cartilages, through the medium of which the ribs are articulated with the ſternum, are united to the ribs with a remarkable obliquiry, as is ſeen in *Fig.* 4, in which *a. c.* is the rib, *c. e.* the cartilage between the rib and ſternum, *e.* the angle formed by their union. This obliquity is ſo formed, that the ribs may be extended and thrown out, deſcribing a circle. The clear and explicit manner in which Mayow has expreſſed himſelf, the full and ſatisfactory reaſoning he has applied to the facts, and the juſt inductions he has drawn, muſt convince the moſt obdurate ſceptic, that he has completely anticipated both Haller and Monro, in their ideas of the operation of the intercoſtal muſcles. We are, by no means, ſo prejudiced in favour of our author, as not to admit, that Dr. Monro has very ably extended the principle of obliquity to the action of muſcles in general. We only wiſh it to be obſerved, that the principle is clearly explained by Mayow, and therefore he muſt be conſidered as the diſcoverer of this important fact in phyſiology. He had, alſo, many opponents to ſtruggle with, in promulgating this doctrine. It was an opinion generally received, previous to his time, that the external intercoſtals, were intended to dilate, and the internal, to contract the thorax, during reſpiration. This doctrine acquired ſtrength, and ſeemed firmly eſtabliſhed, on account of its being countenanced by the firſt phyſiologiſts of the times, as the reader will ſee, by conſulting the twenty-third and twenty-fifth pages of the Pharmaceut. Rational. of Willis. Mayow was aware, that he had to oppoſe not only a prevailing doctrine, but one which ſeemed ſanctioned by the authority of time and experience; for he opens the ſubject with ſaying, that he ſhall not pay ſo much regard to the authority of writers, as to truth itſelf. Notwithſtanding, however, this general prevalence of opinion, that the two layers of intercoſtals performed different offices, Mayow's doctrine, not long after it was publiſhed, acquired its advocates, and thoſe of the moſt powerful kind. In a ſyſtem of anatomy, publiſhed in 1685, the author adopts the opinion, of the co-operation of the intercoſtals, in elevating the ribs.* It is a received opinion, ſays he, that the outward intercoſtal muſcles are miniſterial to the dilatation, and the inward to the contraction of the thorax; but as I humbly conceive, it is more agreeable to reaſon, that both the outward and inward intercoſtal muſcles, do aſſiſt each other, in the dilatation of the thorax; by reaſon both kinds of muſcles have their extremities implanted into the margents of the next upper and lower rib, whereupon the moſt looſely faſtened doth approach the more ſtrongly, by the contraction of the intercoſtal muſcles; wherefore the lower rib, having a more lax articulation than the upper, muſt neceſſarily be drawn upward, by the motion of the muſcles, toward the upper more fixed rib, as to the center of motion; whence it may be reaſonably deduced, that both the external and internal intercoſtal muſcles, though they have different inſertions into the ribs, yet, in their contractions, they aſſiſt each other, at the ſame time, to lift the ribs upwards, toward the throat.* This poſition is proved, by the very ſtructure and ſituation of theſe muſcles; their obliquity is contrived, to give them greater extent of motion; for had they been inſerted at right angles, they would have defeated the purpoſe which nature intended, therefore the grand Architect, hath moſt wiſely contrived the divers ſituations of the external and internal intercoſtal muſcles, that both pulling obliquely, with equal force, might mutually concur to the carrying ribs upwards.† It is rather ſingular, that although theſe paſſages have all the appearance of being a tranſlation from Mayow, yet Collins does not take notice of this: he, however, mentions him immediately after, ſhews great reſpect for his authority, and gives him the diſcovery of the double articulation of the ribs. And it is worthy our remark, he ſays, that the ribs have not only one, as is commonly thought, but two articulations, diſcovered by the learned Dr. Mayow, in his Treatiſe de Reſpiratione.*

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* A Syſteme of Anatomy, treating of the body of Man, Beaſts, Birds, Fiſh, Inſects, and Plants, illuſtrated with many Schemes; by Samuel Collins, Doctor in Phyſick, Phyſician in Ordinary to his late Majeſty, of bleſſed Memory, and Fellow of the King's moſt famous College of Phyſicians, in London, and formerly a Fellow of the Royal Foundation of Trinity College, in the moſt flouriſhing Univerſity of Cambridge. London, 1685.

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* P. 824-5.

† P. 826.

Notwithſtanding the opinion, that both ſets of intercoſtal muſcles conſpired to elevate the ribs, during their action, was afterwards oppoſed by ſome anatomiſts, yet it had abettors of the firſt reſpectability, who all acknowledged it to be the diſcovery of Mayow. Drake, in his anatomy, mentions this diſcovery, attributes it to its right author, and adopts the opinion. The fibres of theſe muſcles (intercoſtals) run in an order contrary to each other, which has made ſome anatomiſts imagine, that they antagonized one another, fancying that the external drew upwards, and the internal downwards. This erroneous opinion, was firſt ſeverally refuted by the learned Steno, and our ingenious Dr. Mayow, whoſe arguments, however convincing, have, nevertheleſs, failed of ſucceſs, over the prejudices of many later writers†. He mentions the miſtake of Willis, with reſpect to the action of the intercoſtals, which, he ſays, is ſcarce worth while taking notice of, were it not for the great authority of the man, which is ſufficient to keep that error in countenance even to this very day. Verheyen, an accurate anatomiſt, at the cloſe of the laſt, and commencement of the preſent century, mentions Mayow's diſcovery of the conſpiring action of the intercoſtals, and adds, that his treatiſe on reſpiration, is very learnedly and ingeniouſly written. He obſerves, (1) That the ribs are ſo conſtructed, that when they are raiſed, they muſt, at the ſame time, be thrown out, and thus widen the cavity of the thorax. To elucidate what he means, he gives a diagram, and deſcribes it, in a manner ſimilar to Mayow. With reſpect to the actions of the intercoſtals, as producing this effect, he adopts the opinion of Mayow, whoſe obſervations, he adds, he always ſound conformable to truth, by his own diſſections. The doctrine, then, that the ribs are thrown out into an arch, while they are at the ſame time raiſed, during inſpiration, and that both ſets of intercoſtals conſpired to produce this effect, as each upper rib is more fixed than the lower, was brought forward by Mayow, and adopted by ſucceding anatomiſts, before Haller or Monro. It appears ſomewhat extraordinary and ſurpriſing, that the obſervations and arguments of Mayow, Collins, Verheyen, and Drake, ſhould have eſcaped the attention of theſe great anatomiſts; and our ſurpriſe is encreaſed, when we conſider, that both Monro and Haller quote the words of theſe authors, who have taken conſiderable pains, to confirm Mayow's illuſtration of the beautiful mechaniſm of the thorax*.

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* P. 826.

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† Anthropoligia Nova, or a new Syſtem of Anatomy, by James Drake, M. D. Fellow of the College of Phyſicians, and F. R. S. London, 1717. P. 400. Vol. II. We wiſh the reader would conſult the account which Drake has here given, as the paſſages are much too long to tranſcribe.

(A) Muſculorum contractionem, a particulis diverſi generis, in muſculi compage invicem commixtis,

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* Mayow is quoted by Haller, Elem. Phyſ. 3-206. and the reader is referred to ſeveral paſſages in Drake's Anatomy, by Monro, in his publication de Venis Lymph. Valv. p. 80. Edin. edit.

mixtis, mutuoque efferveſcentibus, provenire ſtatuam. p. 8. Tractat. Quartus. Spirituum animalium influxum, ad functionem motivam obeundam neceſſarium eſſe, minime dubitandum eſt: in quantum ſc. nervo præciſo vel obſtructo, muſculus, in quem idem diſtribuitur, contractionem inire prorſus nequit, cæterum non eſt putandum, muſculorum contractionem a ſpiritibus animalibus ſolis dependere; utpote cui peragendæ particulis inſuper aliis a ſanguinis maſſa ſuggeſtis, omnino opus eſt. Quandoquidem enim, ſanguis arterioſus continuo gurgite, eoque pleniore, quam ad cæteras partes, aut quam eorum nutritioni ſufficit, præſertim in motibus violentioribus, ad muſculos appellit, liceat concludere, ſanguinem arterioſum nonnihil ad muſculorum contractionem neceſſarium, in tranſitu ſuo deponere. P. 15-17.

(B) Quod ſpectat ad naturam particularum motivarum, a cruoris maſſa ſecretarum, noſtra fert opinio, eaſdem indolis ſalino-ſulphureæ eſſe. Etenim anotare eſt, pinguedinis jacturam haud modicam in exercitiis violentioribus fieri, eamque in laboribus diuturnis pene totaliter abſumi: cum tamen e contrà, animalia otio indulgentia, et a laboribus vacantia, admodum obeſa evadunt, eorumque muſculis adeps copiâ ſatis amplâ accreſcit. Unde colligimus, particulas ſanguinis ſulphureas, è quibus pinguedo componitur, in muſculari contractione obeundâ partes aliquas obtinere. P. 22-3.

(c) Animalia, in exercitiis violentioribus, veluti in curſu valde concitato, omnino neceſſe habeant reſpirationem quam maxime intendere; quod propterea fieri videtur, quoniam in motibus violentis, particulæ nitro-aereæ, pro muſculorum contractione inſtituendâ a ſanguinis maſſa ſecernuntur et abſumuntur, p. 24. Nempe exiſtimo, particulas nitro-aereas, a cerebro in partes motrices proſilientes, ibidem cum particulis ſalino-ſulphureis iis in occurſum datis effерveſcere; a quibus mutuo ſe exagitantibus, contractio muſcularis perficitur. Et hinc eſt, quod pro motu animali continuando omnino neceſſe eſt, ut pabulum ſalino-ſulphureum, particulæque nitro-aereæ, in ſaguinis maſſa nunquam deficiant; et quanto intenſius contractio muſcularis inſtituitur, veluti in laboribus violentioribus, tanto majora particularum nitro-aerearum et ſulphurearum diſpendia fiunt; pro quibus reſarciendis, non tantum reſpiratio

ratio intenditur, ſed inſuper alimentum, particulis ſalino-ſulphureis refertum, copiâ majori ſumendum eſt, hinc ea, quæ ſale volatili ſulphureque copioſo conſtant, vires laboribus diuturnis attritas reficiendo, præcipue idonea ſunt. P. 30.

(D) Super hoc, in dubiis aliquandiu hæſitaverim, an non particulæ nitro-aereæ immediate a ſanguinis maſſa in partes motrices cederent: verum cum ſeriò ad rem attenderim, magis probabile viſum eſt, particulas motivas, a ſanguine ſuggeſtas, indolis ſalino-ſulphureæ eſſe; unde ſequitur, particulas nitro-aereas a cerebro provenire, et conſequenter ipſos ſpiritus animales eſſe. P. 32-3. Concedendum eſſe arbitror, ſpiritus animales, in animalibus perfectioribus, non niſi in cerebro elaborari, atque eoſdem ab eo fonte ad ſpinalem medullam, nervoſque ab eâ oriundos diſſeminari. P. 38. Quidni ergo ſpiritus animales potius ab aere, quam alimentis ingeſtis ducantur? Certè aer particulis maxime activis, ſubtilibuſque impprægnatur; illiuſque hauriendi tanta neceſſitas eſt, ut ne momento quidem temporis, ſine eodem, vivere poſſumus. Et quidem immenſa ſpirituum animalium diſpendia aliundè quam ab aere inſtauriri, vix poſſe videntur. P. 45.

(E) Recepta opinio eſt, muſculos intercoſtales tantum exteriores dilatando, interiores autem contrahendo pectori inſervire. At mihi videtur rationi magis conſentaneum ab iis utriſque ſimul pectus dilatari. Nempe affirmare fas ſit, coſtas ſurſum tractas pectoris ſpatium dilatare, deorſum autem retractas idem contrahere. Supponimus enim hic (quod cuivis in ſceleto videre datum eſt) coſtas cum ſpina et ſterno non ſecundum angulos rectos articulari, ſed angulos infra coſtas eſſe paulo recto minores: ita ut ſi coſta ſurſum trahatur, ejus articulationes cum ſpinâ et ſterno, verſus angulos rectos accedant. Aſſerimus inſuper, a coſtis ad angulos rectos elevatis, pectus dilatari. P. 278-9. Quandocunque muſculus duobus oſſibus affixus contrahitur, os minus fixum ad alterum magis fixum accedit: quapropter, cum inferior quæque coſta minus fixa eſt quam ſuperior, neceſſe eſt, ut muſculis intercoſtalibus etiam interioribus ſe contrahentibus, coſtæ ſingulæ inferiores ſurſum trahantur. Imo ſimilis planè ratio obtinet in interioribus muſculis ac in exterioribus; neque obſtat, quod illæ diverſa poſitione coſtis inſeruntur, uti *Fig.* , patet, ubi muſculus interior, *a. a.*, in contractione ſuâ, æque ſurſum trahet coſtam inferiorem, mobilioremque, ac muſculus exterior *b. b.* Advertendum eſt hic loci, quod coſtæ cum ſpinâ ita articulentur, ut coſtæ a muſculis prædictis tractæ, facile aſcendant, et in orbem eleventur. P. 281.

(F) Et hoc ulterius adhuc oſtendit muſculorum intercoſtalium obliquus et contrarius ſitus. Ideo enim videtur natura muſculos illos oblique coſtis inſeruiſſe (quanquam iiſdem ſurſum aut deorſum movendis recta inſertio melius conveniret) quia coſtarum interſtitia adeo minuta ſunt, ut ſi muſculi iſti rectis angulis insererentur, breviores eſſent, quam ipſa muſculorum natura patitur; quapropter, ut dicti muſculi juſtam longitudinem obtinerent, eos oblique, ut fit, coſtis inſertos eſſe oportet: cum tamen obliqua hæc poſitio ad coſtas ſurſum movendas minus idonea ſit, ideo natura, machinatrix ſapientiſſima, diverſi ſitûs muſculos conſtituit, ut dum hinc inde æquali nixu oblique coſtas trahunt, coſtæ interim rectæ ſurſum aſcendunt, prout in *Fig.* , oſtenditur; ubi muſculis exterioribus *a. a.*, et interioribus *c. c.*, ſimul ſe contrahentibus, coſta inferior, mobiliorque non oblique ſed rectà ſurſum aſcendet; perinde ac ſi a maſculo, rectis angulis ei affixo, traheretur. Plane ut videantur muſculi interiores, ſimul et exteriores, eodem tempore ſe contrahere, et ſociato nixu coſtas ſurſum ducere, pectuſque ampliare. P. 282-3.

(G) Advertendum eſt hic loci, quod coſtæ non unicâ, uti vulgò creditur, ſed duplici articulatione cum ſpinâ conjungantur; articulique iſti adeò oblique collocentur, talique artificio formentur, ut coſtæ a muſculis intercoſtalibus ſurſum trahi nequeant, quin eædem ſimul extrorſum pro majori pectoris dilatatione trahantur, prout in *Fig.* . manifeſtum eſt. In quâ *a. e. i.* coſtæ portio ſit, cujus caput rotundum *a.* ſinum in ſpinâ excavatum *e.* ingreditur, quæ articulatio ſuperior et interior eſt. E contra vero, in articulatione alterâ, ſc. inferiori et exteriori, ſinus, is autem minus conſpicuus in coſtâ ad *e.* excavatur, qui cum protuberantiâ ſpinæ, ad *b.* extante articulatur. Jam vero ſi ſupponamus caput coſtæ iſtius *a.* in cavitate ſpinæ *c.* collocari, et ſinum coſtæ *e.* protuberantiæ ſpinæ *b.* incumbere, et dein coſtam eam, binis iſtis articulationibus ſpinæ connexam, ſurſum moveri, facile eſt conceptu, coſtam eam ad lævam, ſive quod idem eſt, reſpectu pectoris extrorſum latum iri. P. 284-5.

(H) Neque illud prætereundum eſt, quod cartilagines iſtæ, quarum interventu coſtæ cum ſterno conjungantur, non ſine obliquitate notabili coſtis inſerantur, prout in *Fig.* . delineatur, in quâ *a. c.* coſta ſit, *c. e.* cartilago, cujus interventu coſta ea cum ſterno combinatur, *e.* angulus ex commiſſura ambarum conſtitutus. Obliquitas autem iſta eo ſpectat, ut coſtæ extendantur, et extrorſum in orbem trahantur. P. 285.

(I) Verum rem penitus inſpicienti et arcuatam coſtarum figuram, earumque inarticulationem conſideranti manifeſtum eſt, coſtas attolli non poſſe, niſi ſimul recedant extrorſum, atque ita thoracem amplificent. P. 310, Vol. II.

SECT. XII.

Of DIGESTION *and* USE *of the* SPLEEN.

When we reflect upon the many diſeaſes, idiopathic and ſympathetic, to which the ſtomach is ſo often ſubject, we are not ſurpriſed, at the almoſt innumerable diſſertations that have been publiſhed, to explain the nature and cauſe of digeſtion; that grand function, upon which depends the life and activity of the whole frame. To an attentive obſerver, nothing appears more curious, than digeſtion, by which ſubſtances the moſt heterogeneous, are converted and aſſimilated into a bland and nutritious fluid. One of the firſt cauſes to which it was attributed, was fermentation, obviouſly becauſe this operation was the firſt that preſented itſelf; in which it was perceived, that the qualities of ſubſtances were ſpontaneouſly and completely changed. It was natural, therefore, to have recourſe to a ferment, which ſeemed ſo ſatisfactorily to explain this important function. The term fermentation, was applied almoſt to any internal motion, which took place in various ſubſtances; it was applied, uniformly, to the decompoſition of all bodies, and was equally extended to the efferveſcence, produced by the union of an acid with an alkali. Decompoſition, certainly, takes place in every fermentation; and in this ſenſe of the word, it is with equal propriety applied to digeſtion.

The knowledge of reſpiration, has beautifully explained the connection between that function and digeſtion. That animal food increaſes the neceſſity, and cauſes a greater abſorption of oxygen by the lungs, is now placed beyond a doubt, and will certainly lead to important regulations, in the regimen of patients, in different diſeaſes. Certain it is, that the union of oxygen with hydrogen, &c. produces conſiderable effects in the ſyſtem; and by this union, provided the oxygen be taken in by the lungs, reſults animal heat, the *ſine qua non* of animal life. If the oxygen be thrown in by the ſtomach, in the form of acids, &c. it is in a fixed ſtate, ſeparated from its caloric; of courſe this method of oxygenating the ſyſtem, is not ſo uſeful, when we wiſh to increaſe the generous and benign ſtimulus of animal heat, as in chloroſis, dropſy, &c. But in fevers of the putrid kind, when the heat is very great, from the rapid decompoſition of oxygen, and when, at the ſame time, we wiſh, under certain circumſtances, to throw in that fluid, the ſtomach ſeems, at leaſt, as good a medium as the lungs, for oxygenating the ſyſtem. I, therefore, cannot agree with my ingenious friend, Dr. Rollo, that it is proper, in every caſe, to oxygenate the ſyſtem by the ſtomach. Beſides, when we oxygenate by the lungs, we aid digeſtion more powerfully; and in dyſpepſia, where acidity always more or leſs prevails, oxygen is preſent in ſufficient quantity; but it requires animal heat to give vigour, and apply the oxygen to the purpoſes of the œconomy; and cold extremities is a never failing ſymptom in dyſpepſia. Acids, too, are cooling, and they appear to me to produce this effect, by leſſening the abſorption of oxygen with caloric by the lungs.

Let us now examine, what Mayow has ſaid on this ſubject: He will not admit the opinion of his time, that digeſtion is owing to an acid ferment in the ſtomach. The acid eructations are produced by bad digeſtion, and not by any natural acid in the ſtomach. Iron, indeed, is corroded, and milk coagulated in the ſtomach. How is this to be accounted for? In the firſt place, I ſuppoſe, that digeſtion is performed by the animal ſpirits, carried through the nerves, which are ſo numerouſly ſupplied to the ſtomach; and according as the animal ſpirits are ſupplied, in greater or leſs quantity, to the chylopoietic organs, the digeſtion of our food is more ſlowly or more quickly performed. (A) Hence if any one, after food, applies to ſtudy, or employs his thoughts on any abſtruſe ſubject, ſo that the animal ſpirits are, in a great meaſure, detained in the brain, on account of the attentive contemplation, and greater agitation of the mind, the office of digeſtion is not rightly performed, becauſe there is a want of animal ſpirits: but our food remaining undigeſted in the ſtomach, cauſes a heavineſs and anxiety in that organ, as any one may put to the teſt of experiment in himſelf. But, on the contrary, when the mind is not engaged in thought, as in ſleep, the digeſtion of our food goes on perfectly well; for then the animal ſpirits, not being employed in carrying on other functions, are copiouſly ſupplied to the organs deſtined to digeſtion: hence it happens, that immediately after a full meal, we feel a great propenſity to ſleep. It is a fact, which no one will now diſpute, that the brain ſhould be continually ſupplied with well oxygenated blood, in order that its functions may be exerted vigorouſly, and unimpaired. Certain it is, too, that oxygen powerfully aſſiſts digeſtion; but whether it produces this effect by giving vigour to the nervous ſyſtem, or that the oxygen is immediately carried to the ſtomach, for this purpoſe, from the aorta, through the cæliac and gaſtric arteries, I cannot determine. Mayow, himſelf, had his doubts upon this point. (B) I will not, he ſays, poſitively aſſert, whether in land animals,* beſides the nitro-aerial ſpirit communicated by the nerves, there be alſo an aerial ferment, directly ſupplied from the maſs of blood to the ſtomach. I ſhould

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* He had juſt quoted Dr. Gault. Needham, to prove, that there is a communication between the ſtomach and air-bladder of fiſhes.

I ſhould think, it was neceſſary in both caſes. The ſtomach, we know, is very vaſcular; no function is performed by the blood, without the aſſiſtance of nerves. Is it then inconſiſtent to ſay, that the arterial blood, which follows ſo copiouſly to the ſtomach, is intended to ſupply oxygen, as a component part of the gaſtric juice, and alſo animal heat, as a ſtimulus to the ſtomach;* and that the nerves are intended to give vital energy to the whole, by the ſupply of nervous electricity, or any other power, the reader chuſes to give the nerves? From obſerving, that the brain muſt be ſupplied with fire-air particles, and that digeſtion is injured, if after a full meal we are employed in deep thought; (c) hence, I would conclude, ſays Mayow, the far-famed ferment of the ſtomach, is chiefly compoſed of nitro-aerial particles, which being depoſited, through the medium of the nerves, or the membranes of the ſtomach, it is highly probable, are there mixed with a proper fluid, ſecreted from the blood by glandular membranes,

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* The ingenious Mr. Coleman has ſhewn, that it is animal heat, and not oxygen, which is the ſtimulus to the fibre.

branes, of which the ſtomach is compoſed; both theſe being carried, by proper veſſels, into the cavity of the ſtomach, from its fermenting liquid.* And we may now know, why a clear and pure air cauſes appetite, and promotes the digeſtion of a ſurfeit; while, on the contrary, a denſe atmoſphere, polluted with noxious vapours, ſoon produces anxiety at the ſtomach. (D) If the ſtomach is entirely empty, it is very probable, that its inner membranes are irritated by the nitro-aerial particles; hence hunger is produced. Such is the admirable and conciſe manner, in which the accurate Mayow reaſons on this ſubject. Who can with-hold their applauſe, when we find him, in every caſe, ſtriking out ſome noble truths, and wonderfully anticipating the modern pneumatiſts?

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* Although Mayow had denied, that digeſtion was owing to an acid ferment; yet we ſee, he makes uſe of this term, becauſe the phyſiological vocabulary was not ſufficiently enlarged, to allow a better. This is evident, from what he ſays in the ſentence immediately preceding: Although the nitro-aerial ſpirit is, of itſelf, not acid, yet iron is corroded by it, and by being united with other ſubſtances, it becomes almoſt an univerſal menſtruum: hence the farfamed, &c. &c. Has he not here anticipated Stevens, Spallanzani, and Hunter, in their ideas of the gaſtric ſolvent?

After ſome obſervations on the ſuccus pancreaticus, which, he ſays, contains fire-air particles, the next ſubject which occupies his attention, is the ſpleen; the uſe of which has bafled the inveſtigation of phyſiologiſts. Mayow obſerves, that after this part of his work was written, De Graaf's publication, De Succo Pancreatico, came into his hands, in which the ſame uſe was aſſigned to it by De Graaf, as by himſelf, *viz.* by its union with bile, to form a proper digeſtive for the chyme. But we cannot, adds Mayow, agree with him, that the pancreatic juice poſſeſſes any acidity; for in that caſe, inſtead of forming a homogeneous fluid, it would produce an indiſſoluble coagulum, which acids are known to precipitate from bile*. The ſubſerviency of the ſpleen to the liver, has been long noticed by anatomiſts, and conſidered as its principal, if not its only uſe; notwithſtanding that the idea is contradicted by the remarkable and well authenticated fact, that animals can live, perfectly well, after the ſpleen has been cut out, as is obſerved by Mayow, Baglivi, and innumerable others. The ingenious Dr. Saunders has made an experiment, which deciſively proves, that the functions of the liver are, altogether, independent of the ſpleen. He examined the bile of a dog, from which the ſpleen had been removed for ſeveral weeks, and found no difference between it and bile under the uſual circumſtances*. When it was found, that the animals could live without a certain viſcus, no wonder that the medical world was perplexed about its uſe. It is imagined, that a phyſiologiſt, now a lecturer in the Borough, has diſcovered (if it is diſcovered) the true uſe of the ſpleen. It is conſidered by this gentleman, as a reſervoir for blood, which is ſupplied to the chylapoietic organs, during digeſtion. The ſtomach being diſtended with food, preſſes upon that viſcus, and prevents the blood from flowing freely through the ſplenic artery; conſequently a greater than uſual quantity will be carried to the ſtomach and pancreas, into which branches from the ſplenic artery are diſtributed : hence a greater ſecretion takes place, from theſe viſcera producing their ſalutary effects. If this be the whole of the theory, Dr. Haighton will find, that he has been anticipated, many years ago. We ſhall firſt examine what Mayow has ſaid upon this ſubject.

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* For a confirmation of this fact, ſee Maclurg's Experiments on Human Bile, p. 11; and Saunders on the Liver, p. 102-3.

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* P. 42.

That it may be underſtood, what opinion we entertain, reſpecting the uſe of the ſpleen, we muſt here repeat, what has been before mentioned, *viz.* It is probable, that to ſupport animal life, it is neceſſary that the animal ſpirits, *i. e.* the nitro-aerial particles, muſt be continually paſſing through the brain, or, at the leaſt, the cerebellum; hence it happens, that if reſpiration be ſuppreſſed, or the pulſation of the heart be interrupted, but for a moment, the more perfect animals are inſtantly deſtroyed. The nitro-aerial particles are carried by nerves from the brain to the different parts. The organs of digeſtion, require a greater ſupply of nitro-aerial particles at one time, than at another. It is, therefore, probable, that the ſpleen is formed to mix, intimately, the nitro-aerial particles with the blood. The functions, therefore, of the ſpleen are theſe: (E) 1. That the nitro-aerial particles, which inceſſantly flowing through the brain, are not all exhauſted in the natural and animal functions, might be carried back into the maſs of the blood, and intimately united with the ſame. 2. That the nitro-aerial particles might be properly and regularly ſupplied to the digeſtive organs; for as theſe organs are, more or leſs, full, ſo they require a greater or leſs ſupply of fermentative particles. 3. That the nitro-aerial particles, endued with a motion and activity, and by being intimately blended in the ſtructure of the ſpleen, with the ſalino-ſulphureous particles, might produce, in the maſs of blood, an effervescence, of ſuch a nature, as ſhall exalt its ſalino-ſulphureous particles to a proper volatility. Mayow, we ſee, conſidered the ſpleen, as ſubſervient to digeſtion, not mentioning a ſingle word of its connection with the liver. He makes uſe of the term nitro-aerial particles, inſtead of arterial blood, as it is only in it, that the fire-air is conveyed to the different parts of the ſyſtem.* In a work publiſhed in 1723, entirely on the ſpleen, we find the author maintaining, that it is intended to aſſiſt digeſtion*. It is the fulleſt and beſt account of the ſpleen we have ever read; and the author ſeems to have beſtowed very conſiderable pains upon the ſubject. After mentioning the opinion of the antients, reſpecting its nature and uſe, and giving a very accurate deſcription of it, he then offers, very fully his ideas, as to the functions it performs. From the ſtructure, he calls it an animal ſponge, the great reſervoir and magazine of blood. Mentioning the numerous veſſels ramifying through it, he obſerves; this contrivance in nature, can ſerve no other purpoſe, than moſt effectually to form a ſponge, which, when filled by arterial blood, can, at pleaſure, upon its contraction, throw it all out again, into the great channel of the ſplenic vein, or back into the artery, if it be not too full†. The great quantity of blood, which flows to this viſcus, muſt have been intended, by nature, to anſwer

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* The fire-air, carried by the nerves, was modified, and became the animal ſpirits.

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* Of the Spleen, its Deſcription and Hiſtory, Uſes and Diſeaſes, by William Stukely, M. D. C M L. and S R S. London. 1723.

† P. 19.

ſome important purpoſe. The ſpleen, then, muſt be ſaid to officiate to the ſtomach, by pouring in upon it, as it ſwells in eating, a new quantity of blood, beſides that derived in the ordinary courſe of circulation, which is to procure the required heat and neceſſary juices in concoction. And that this is not only neceſſary, but likewiſe in a very high degree, a little conſideration will perſuade us*. It would occupy too great a portion of the preſent work, to mention the facts and arguments, which Stukely brings forward, in confirmation of his opinion; ſuffice it to ſay, that it was his opinion, that it ſupplied blood, during digeſtion, to all the chylopoietic organs, ſtomach, pancreas, &c†. and adds, Mayow's opinion quadrates with ours, only putting blood, inſtead of his favourite nitro-aerious particles, which in due plenty, and with a certain regimen, are carried to the bowels dedicate to the concoction of the aliment‡.

(A) Hinc ſi quis mox a paſtu ſtudiis incumbat, aut ſuper materiâ difficili cogitabundus fuerit,

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* P. 33. † P. 61. ‡ P. 42.

ita ut ſpiritus animales in cerebro, ob contemplationem attentam et majorem mentis agitationem, magnâ exparte detineantur, digeſtionis munus, ob ſpiritum animalium defectum, rite non perficitur; ſed cibus in ſtomacho indigeſtus manens gravedinem et moleſtiam ventriculo facit, uti quilibet in ſeipſo experiri poteſt; cum tamen, e contra, dum animus e cogitationibus vacat, uti etiam in ſomno, ciborum concoctio optimè peragitur; in quantum ſc. ſpiritus animales, non jam in functionibus aliis obeundis verſati, ubertim per viſcera digeſtioni dicata deferuntur. Huc etiam ſpectat, quod mox a prandio, ſive cœnâ pleniore, dormiendi cupiditate teneri ſolemus. P. 54-5.

(B) Utrum vero etiam in animalibus terreſtribus, præter ſpiritus nitro-aereos nervorum ductu advectos, fermentum inſuper aereum immediate a ſanguinis maſſa in ventriculum faceſſat, nihil certi ſtatuere poſſum. P. 56.

(C) Quapropter ſtatuendum eſſe videtur, fermentum ventriculi adeo decantatum, a particulis nitro-aereis præcipue conſtare, quæ nervorum ductu in membranas ventriculi depoſitæ, ibidem, uti veriſimile eſt, liquori idoneo, membranarum glanduloſarum, quibus ventriculus conſtat, miniſterio, a ſanguine ſecreto, commiſcentur; e quibus utriſque, per vaſa peculiaria, in ventriculi cavitatem ingeſtis, liquor ejus ſermentativus componitur. P. 55-6.

(D) Si ſtomachus cibis prorſus vacuus fuerit, illius membranæ interiores a particulis nitro-aereis, uti veriſimile eſt, vellicantur; unde fames oriri videtur. P. 57.

(E) 1º Ut particulæ nitro-aereæ, quæ cerebrum ſerie continua pertranſeuntes, in functiones naturales aut animales non erogantur, in ſanguinis maſſam reportentur, eidemque rite admiſceantur. 2º Ut particulæ nitro-aereæ debitâ copiâ et cum regimine quodam ad viſcera ciborum coctioni dicata ferantur; prout enim viſcera ea alimento referta, aut vacua fuerint, ita eadem, majori aut minori, particularum fermentativarum affluxu opus habent. 3º Ut particulæ nitro-aereæ, in motu et vigore poſitæ, cum particulis ſanguinis ſalino-ſulphureis in lienis compage quoad minima permixtæ, efferveſcentiam iſtiuſmodi in cruoris maſſa excitent, quæ ad particulas ejus ſalino-ſulphureas ad juſtam volatilitatem perducendas idonea eſt. P. 62-3.

SECT. XIII.

MAYOW'S EXPLANATION *of ſome* DISEASES *from his* DOCTRINE.

When a principle has been diſcovered, which produces conſiderable effects in the phænomena of nature, it becomes a matter of courſe, not only to recur to this principle for explanation, in deviations from regularity, but alſo, upon the ſame foundation, to extend our enquiry by analogical reaſoning. Thus, ſoon after Dr. Prieſtley had made his diſcovery of dephlogiſticated air, he immediately extended his ideas to reſpiration; and the diſcoveries of the great Lavoiſier, in France, were followed up, by their application to the irritability of the fibre. When Mayow, therefore, obſerved the univerſal agency of his fire-air particles, his penetrating mind immediately perceived, that they would aſſiſt him in explaining the ſymptoms of diſeaſe. In our days, upon the ſame foundation, and in an almoſt ſimilar manner, a ſyſtem of phyſic has been formed, which certainly explains ſatisfactorily, ſome of the moſt obſcure laws of the animal œconomy. Upon our knowledge of the airs, a ſuperſtructure is now daily riſing, which will add further dignity to the profeſſion of phyſic, and will render it a purſuit, more philoſophical and engaging: and we would fain indulge a pleaſing thought, that by a more general diffuſion of its principles, the credulity of mankind will not be ſo much impoſed upon, by that numerous herd of empirics, which every where inſult the common ſenſe of the metropolis. The materia medica has been ſo much enriched, by the new diſcoveries, that the ſtudy of chemiſtry muſt now form an indiſpenſable part, in the education of a medical practitioner. The beautiful and uſeful manner, in which the new diſcoveries illuſtrate the nature of diſeaſe; and the remedies they have ſuggeſted, in caſes hitherto incurable, will render the doctrine, founded upon the pneumatic theory, a moſt powerful aid in the hands of the phyſician.* The innumerable caſes, already publiſhed, by Dr. Beddoes, afford a convincing proof of the utility of the airs, in a variety of complaints, which had reſiſted the effects of the uſual remedies; and in the hands of a ſkilful and cautious practitioner, I have no doubt, that this branch of ſcience, in conjunction with remedies by the ſtomach, will be made ſubſervient to the moſt uſeful purpoſes in the practice of phyſic. The manner in which the airs act upon the animal frame, did not eſcape the all-inveſtigating eye of Mayow. His explanation of their effects upon the body, is, in ſome caſes, perfectly analogous to that of modern times. The chapter, in which he handles this ſubject, is entitled, 'Of the diſeaſes, to which the animal ſpirits are ſubject.' The firſt diſeaſe which arreſts his attention, is phthiſis; and this, we know, was the firſt complaint, in which the theory and practice of pneumatic chemiſtry was applied. He explains it, as ariſing from the want of fire-air: this forms, however,

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* I here allude, more particularly, to the cure of diabetes,

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diabetes, by the hepatiſed ammonia, &c. and the venereal diſeaſe, by the nitrous acid of oxygenated muriat of potaſh, for which ſee the ingenious work publiſhed by Dr. Rollo, and Mr. Cruikſhank of Woolwich.

no objection to the juſtneſs of his remarks, as the reaſons which he ſubjoins are concluſive, and which occurred to Dr. Beddoes, in his obſervations on that diſeaſe. (A) In order that animal life may be properly ſupported, it is of great conſequence, that the lungs poſſeſs a ſound diathesis, that the nitro-aerial particles may be ſupplied to the blood, in ſufficient quantity, and be intimately united with its ſulphureous particles. Hence, if the lungs ſhould become conſumed or tabid, the blood, on account of the want of nitro-aerial particles, is not rightly fermented; and becauſe the *motive* particles of both kinds are deficient, there will follow a proſtration of ſtrength, and a waſting. This theory, we ſee, is oppoſite to the one which Dr. Beddoes propoſed, ſome years ago;* but let it be remembered, that the firſt trial made, in this complaint, was by the inhalation of oxygen gas. It ſpreads flowers, ſays Mr. Chaptal, on the borders of the tomb, and prepares us, in the gentleſt manner, for the laſt dreadful effort of nature.*

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* I however think it probable, that inſtances have occurred, where the loſs of a conſiderable part of the ſubſtances of the lungs, has checked the progreſs of the diſeaſe. Obſ. on Calculus, &c. p. 146.

That oxygen is one of the moſt powerful ſtimuli, we can apply to the irritability of the fibre, is, I believe, now very little doubted. When we know, that an animal cannot exiſt one moment of time, without a ſupply of this neceſſary fluid, we cannot but conclude, that it is concerned, in a variety of alterations which occur in the body, both in health and diſeaſe. That its quantity may be increaſed or diminiſhed, by certain alterations in the ſtate of the ſyſtem, affecting the lungs, is as probable and certain, as that the abſorption by the ſkin, undergoes theſe variations, from a multiplicity of cauſes. It is probable, too, that even ſhould a ſufficient quantity of oxygen be abſorbed by the lungs, yet, from certain cauſes, it may not be ſupplied to different organs of the body, in the quantity which the part may require. The brain is one of thoſe organs, which require a continual ſupply of well oxygenated blood, to ſupport both mental and corporeal vigour. (B) If the nitro-aerial particles, ſays Mayow, are not ſupplied to the brain in ſufficient quantity, or are unable to paſs obſtructed nerves, a deranged ſtate of the animal œconomy muſt be the conſequence; and it would ſeem, that from this cauſe, not unfrequently are produced apoplexy, palſy, and diſeaſes of that kind. If there be a fluid, neceſſary to the functions of the nervous ſyſtem, it is highly probable, that a deranged ſtructure of the nerve will impede, or totally prevent its tranſmiſſion. The influence, termed animal electricity, is, we know, varied in its tranſmiſſion through the nerves, by a variety of cauſes. If, therefore, we agree with Valli, in ſuppoſing the animal electricity to be the ſame with the nervous power, may we not be allowed to imagine, that a nerve may be ſo affected, as to become a non-conductor to that fluid? Is any one air a better conductor than another? Air, we know, is only a conductor, in proportion to the moiſture it contains; but has it been proved, that none of the airs are poſſeſſed of a conducting power? As charcoal is a conductor, it is probable, that the carbonated hydrogen poſſeſſes that quality. Experiments, inſtituted upon theſe queries, would probably be more than curious. That oxygen is of as great uſe to the nervous, as to the muſcular parts of our bodies, is evident, from the great quantity diſtributed by the carotid arteries. A ſixth-part of the whole maſs of blood is driven to the brain, from the heart, in an oxygenated form, and quickly returns thence, de-oxygenated, and is as ſpeedily ſupplied by freſh oxygenated blood. The oxygen, therefore, which is carried to the brain, may, previous to its being applied to the nervous ſyſtem, undergo, let me ſay, from morbid action, combinations, inconſiſtent with the healthy functions of the brain. Does not ſomething, analogous to this, evidently take place in diabetes? Is there not here, diſeaſed action, forming combinations, from the elementary part of our food, inconſiſtent with health? If, in other parts of the body, oxygen enters into morbid combinations, it is probable, that the ſame effect may be produced in the brain. It was maintained by Mayow, that as fire-air was ſo indiſpenſable to the health of the brain, ſo any quantity of another principle, would produce oppoſite effects. He imagined, that a greater than uſual quantity of ſalino-ſulphureous particles, might get admiſſion into, and injure the brain. Whatever obſtructs oxygen, will, in ſome degree, increaſe the other principles; for as oxygen is withdrawn, the other principles will be more or leſs diſengaged, and thus an accumulation take place, as happens in ſcurvy. We do not mean, that they are completely diſengaged in the vaſcular ſyſtem, but that they form a combination, into the compoſition of which, oxygen does not enter, and thus produce the ſalino-ſulphureous particles of Mayow.

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* Elements of Chemiſtry, Vol. I. p. 138-9.

As the nitro-aerial particles, ſays our author, are abſolutely neceſſary to the functions of the brain, ſo the ſalino-ſulphureous ought to be excluded from it. (c) If it ſhould ſo happen, that they get entrance too copiouſly into the brain, the animal œconomy is diſturbed, as happens in intoxication, epilepſy, and ſimilar diſeaſes: for liquors replete with a volatile ſulphur, as ſpirit of wine, and the chemical oil of vegetables, too inconſiderately taken, not unfrequently produce theſe diſeaſes. Intoxication, we know, is produced quickeſt by the ſtrongeſt ſpirits, that is, by thoſe which have the ſmalleſt quantity of oxygen in their compoſition, and their other principles in the looſeſt ſtate of combination. The degree of intoxication, independent of the quantity of ſpirit taken, will, no doubt, be relative to the excitability of the ſyſtem. It is not the mere increaſe of vaſcular action, nor the ſtimulus, abſtractedly conſidered, applied to the ſtomach, which cauſes drunkenneſs. That a de-oxygenated ſtate takes place, is evident, from the paleneſs and laſſitude which ſucceed a debauch. With reſpect to epilepſy,* we know it to be produced by a variety of cauſes, affecting the nervous ſyſtem; but more particularly, that great ſource of nervous energy, the brain. We are very little acquainted with the preciſe nature of diſeaſes, cauſed by affections of this organ. Its excitement and collapſe, to uſe the words of the great Dr. Cullen, depend ſo much upon, what appear to us, contradictory cauſes, that they have divided and perplexed the medical world. Spaſmodic affections of all kinds, are frequently very much beyond the reach of medical art, notwithſtanding the elucidation that has been given to the ſubject, by an ingenious phyſician. Pathology, ſays Dr. Beddoes, is, on no account, more indebted to the author of Zoonomia,

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* The mention of this diſeaſe, brings to my recollection, a caſe which occurred, under my own obſervation. At the commencement of the winter, 1796, when I was in London, a man applied for advice for fits, to which he was ſubject. I mention this, as one of the innumerable impoſitions practiſed by the empirics. I found the complaint to be epilepſy. The ſubject was a ſhoemaker, ætat. 23: had been under the care of an apothecary in town; but being impatient, and allured by the deceitful hand-bills daily diſtributed in the ſtreets of London, he applied for relief to the author of one of them, who told him, that the diſeaſe was trifling, and ſhould be cured in a week; after ſome months, he diſmiſſed him uncured, not, however, before he had

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had taken from him twenty guineas ! ! ! When he applied to me, he had a fit almoſt daily, attended with conſiderable ſickneſs and vomiting at times. I concluded, that the diſeaſe was owing to a diminiſhed energy, connected with dyſpepſia; but I knew, that although I removed the original cauſe, the diſeaſe would be continued from habit. I, therefore, ordered an æther draught to be taken, whenever he, in the leaſt, ſuſpected a fit was coming on. This, together with occaſional vomits, a proper regimen, and pills formed of the calx of zinc, and tartarized iron, reſtored him to health.

nomia, than for the light he has thrown upon the obſcure origin of convulſive diſeaſes. The reader would do well, to conſult this part of Dr. Darwin's publication, with the caſes publiſhed in the fourth part of Conſiderations on Factitious Airs, with the ingenious ſpeculations of Dr. Beddoes on the ſubject.

We have mentioned it, as the opinion of Mayow, that an accumulation of thoſe principles in the ſyſtem, which exclude oxygen, proves highly injurious to the well being of animal life. Some obſervations which he adds, are extremely curious, and are analogous to a ſimilar ſubject, mentioned in Medical Extracts. I wiſh the reader to attend to this explanation, as it decidedly points out, Mayow's ideas of chemical combinations, which ſometimes takes place in our frame. (D) To the ſame cauſe, may be referred a fact, which has more than once come under my obſervation, *viz.* I have known ſeveral ſubject to be ſeized by maniacal and convulſive fits, whoſe ſaliva, when thrown upon the fire, during theſe paroxyſms, has burſt into a flame, like oil, or rather like gunpowder. It is probable, adds Mayow, that theſe phænomena are owing to the ſalino-ſulphureous particles; the reader will recollect, that theſe are the principles which Mayow had maintained, united with the nitro-aerial particles, or oxygen, to produce fire or flame. In the firſt Vol. p. 92, of Medical Extracts, are ſome curious facts, mentioned with a ſimilar idea. Some remarkable caſes are there quoted, from authors of the firſt reſpectability, of perſons who have been conſumed by fire, in conſequence of dram-drinking; ſo eager, ſays the ingenious Dr. Beddoes, are the principles, of which ſuch perſons are compoſed, to combine with oxygen. The reader may attach what credit he pleaſes to ſuch facts. Perſons, who conſider the ſubject with a vulgar eye, will probably turn from it with contempt and ridicule; but the philoſophic enquirer, although he may have his doubts, reſts not his diſbelief upon the appearance of improbability. A fact, ſomewhat ſimilar to thoſe we have referred to, I find mentioned by Baglivi: On diſſecting a dog, the vapors which exhaled from the viſcera, crepitated like ſea-ſalt, when a candle was applied*.

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* Diſſert. de Experim. Anat. Pract. p. 110. The hog

It appears, from ſome obſervations of the ingenious Mayow, that it was his opinion, that the functions of the brain might be injured, not only by directly with-holding the nitro-aerial particles from that organ, but alſo indirectly, by any changes which might be produced, with this principle, in other parts. Is not this evident, from the following expreſſions? (E) Melancholy and madneſs, are owing to the ſalino-ſulphureous particles being detained in the ſpleen, and there effervefcing, for a long time, with the nitro-aerial particles, produce the atra-bilious and gloomy diſpoſition. The manner of his expreſſion here, and the mode of explanation, are conformable to the idea he entertained, of the nature and functions of ſpleen; and the atrabilis was a conſtant attendant of the melancholia, according to the ancients. The curious ſympathy, that exiſts between the brain

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had been, previouſly, fed, with a view to experiment. The lacteals were copious and tinged upon the meſentery, and alſo ſome lymphatics; the fluid in the latter, I taſted, ſays Baglivi, which was very ſharp, and left a very diſagreeable impreſſion upon the tongue; eratque mordaciſſima, et linguam graviter feriebat. The animal was very much emaciated, and laboured under the mange.

and abdominal viſcera, is known to the moſt ſuperficial medical obſerver; but why an affection of theſe parts, produces a concomitant change in the functions of the brain, is difficult to prove. When the chylo-poietic organs are debilitated, or deranged in their actions, who has not obſerved a great diſpoſition in the patient to magnify his complaints? Every medical practitioner muſt have frequently obſerved, that a cathartic has removed that gloomineſs, which overcaſts the minds of thoſe, who are ſubject to what is called the melancholic temperament.

It is evident, that animal heat, the reſult of this decompoſition of oxygen, is abſolutely neceſſary to the energy of the brain. As this is the caſe, we may be indulged in an obſervation. When any of the chylo-poietic organs are in a deranged ſtate, a diſpoſition to the generation of acidity is always preſent; hence oxygen is abſorbed, in leſs quantity, by the lungs; animal heat is, conſequently, diminiſhed, the vaſcular ſyſtem becomes weakened in its action, for the want of a ſufficient ſtimulus, the brain is deprived of its neceſſary quantity of animal heat, and the train of thought will be affected, in proportion as the organ undergoes any modification, from an *altered* ſupply. This is entering more into ſpeculative theory than we wiſh; but when principles cannot be rendered cogniſable to our ſenſes, it is natural and reaſonable, to hazard conjecture; and from conjectures, facts often ariſe, which are perfectly reconcilable to truth and experience. It is not my wiſh nor intention to vindicate every theory, Mayow may have thought his diſcoveries ſuggeſted: but his hints, as well as his poſitive facts, ought to be taken into the account; and from the remarkable co-incidence between his diſcoveries and obſervations, and thoſe of modern times, it would be an injuſtice not to pay as particular attention to his ſpeculations, as to thoſe of a more recent theoriſt upon the ſame ſubject.

Notwithſtanding that oxygen is ſo eaſily taken in by the lungs, it is not decompoſed from the blood by ſimple chemical affinity alone. The heat of the body is, by no means, ſufficient to form the different combinations, according to the laws of chemiſtry. We muſt, therefore, look out for ſome other aid, to account for thoſe various decompoſitions, which we obſerve to take place daily in our body. We can refer it to nothing, but the principle of life. A certain action is, therefore, neceſſary in the veſſels, to prepare the different elementary principles, in order to form the various compounds which the ſyſtem may require. Our food, which conſiſts principally of hydrogen, azot, and carbon, muſt, therefore, be modified, by the powers of the ſyſtem, in ſuch a manner, that when decompoſed, the elementary parts may readily unite with oxygen; a circumſtance abſolutely neceſſary to animal heat and motion. The ſame idea preſented itſelf, to the all-graſping mind of our moſt accurate author. (F) It is abſolutely neceſſary to the ſupport of life, ſays Mayow, that the ſalino-ſulphureous particles* be ſo managed by the powers of the ſyſtem, as to be brought to that ſtate, in which they readily unite with nitro-aerial particles. As it is evident, that ſuch an action muſt belong to the ſyſtem, and as that action may be rendered irregular, by a variety of cauſes, it follows, that the elementary principles may be formed into compound, inconſiſtent with the health of the ſyſtem. (G) If the ſalino-ſulphureous particles, ſays our author, are not properly prepared, on account of ſome morbid diatheſis of the muſcular fibre, an impediment to motion will be the conſequence; and, therefore, it is probable, that the ſpontaneous laſſitude and diſinclination to move, which take place in ſcurvy and jaundice, ariſe from this cauſe. Is there not here, ſomething more than a hint of that theory, propoſed by Dr. Beddoes, and confirmed by Dr. Trotter? and do not the remarkable inability, and diſinclination to motion in ſcurvy, confirm the ideas of Mayow*? with reſpect to jaundice, every medical obſerver is acquainted with the languor, that attends an obſtructed flow of bile into the inteſtines. We have mentioned, that when a debilitated action takes place in the abdominal contents, a conſequent effect is produced upon the brain; and, in proportion as this is affected, a dimiſhed energy

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* The reader will recollect, that Mayow ſaid, that our ſood is compoſed of theſe particles.

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* Vide Lind, Roupp, Blane, &c. A diſinclination to exertion, is ſo prominent a ſymptom, that Cullen mentions aſthenia in his Definition. Synop. Noſolog. Method. P. 294.

is communicated to the whole nervous ſyſtem; or, to uſe the words of Dr. Saunders, a torpor is diffuſed, by ſympathy, over every part of the ſyſtem, and langour and laſſitude prevail.* As a certain degree of vigour is neceſſary in the body, to eliminate and apply the elementary principles, taken in by the ſtomach and lungs, it follows, that whatever leſſens that neceſſary degree of power, prevents the proper decompoſition of our food; the caloric will, therefore, not be diſengaged from the oxygen, and the ſyſtem will want that indiſpenſable ſtimulus to its excitability: hence follow the cold extremities, langour and ſallowneſs of countenance, obſerved in thoſe who are, from any cauſe, affected with torpor of the alimentary canal. There ſeems, obſerves Dr. Saunders, much ſympathy between the brain and liver; and in maniacal perſons, there is generally a defect in the ſecretion of bile.†

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* P. 123.

† P. 154. Does not this explain, the good effects that are ſometimes produced by ſtrong purgatives in mania? Does the ſtramonium act in any other way, than by producing a flow

Whatever part of the works of Mayow we examine, we always find ſufficient cauſe for admiring the fertility of his genius, and the accuracy of his remarks. (H) As it appears, that with debility, as in ſcurvy, there is a deficiency of fire-air, therefore, ſays Mayow, we now know, the reaſon why exerciſe of the body contributes ſo much to the cure of ſcurvy and jaundice, and to keep off the paroxyſms of intermittents: for, during exerciſe, reſpiration is ſo much quickened, that the nitro-aerial particles are thrown into the blood, in much greater quantity. How beautifully has the truth of this theory been illuſtrated and confirmed, in ſome of the moſt popular and recent publications. The reader has only to conſult the works of Beddoes, Thornton, Trotter, &c. to find analogous theories explained and confirmed. In every caſe of muſcular contraction, there is an expenditure of oxygen, and the waſte is in proportion to the quantity and violence of exertion. It is probable, too, that the exertion of

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a flow of the different ſecretions into the inteſtines, and thus relieving their torpor, and ultimately the brain?—Vide Ruſh's Eſſays.

of the muſcles may be ſo violent, as to expend or eliminate out of the body, more oxygen than the lungs are able to convey to the ſyſtem, eſpecially if they be, in any degree, morbidly affected. This ſeems to explain, ſays Beddoes, an obſervation, that has ſo frequently been made at ſea; that the ſcurvy makes its appearance after a ſtorm, when the ſeamen, having undergone violent exerciſe, have expended a great part of the oxygen of the ſolids.* Animals, during violent motion, obſerves Mayow, breathe very quick; becauſe the blood, returning to the heart, is deprived of the nitro-aerial particles, and very much ſo in the brain, for the formation of animal ſpirits. (1) From this we may ſee, the reaſon why blood, drawn during a convulſive paroxyſm, is apt to be thick, and in ſome degree grumous: for in the more violent muſcular contraction, there is not only a very great waſte of nitro-aerial, but alſo of ſalino-ſulphureous particles (upon the proper union of which, the fermentation and fluidity of the blood depend); and on this account it is, that the blood is more diſpoſed to coagulate. This happens too more readily, in theſe caſes, if the parts, ſubſervient to reſpiration, become convulſed, then on account of reſpiration being almoſt ſuppreſt, the waſte of nitro-aerial particles, cauſed by the convulſions, cannot be ſo quickly reſtored, as in other violent motions, when reſpiration is free. The great diſpoſition which the blood ſhews to coagulate, in a de-oxygenated ſtate, and the curious, but extraordinary caſe, furniſhed to Dr. Beddoes, by the Leyden Profeſſor, are a confirmation of theſe admirable obſervations of Mayow. The reader will recollect, too, how quickly oxygen communicated fluidity to the coagulated blood, in an experiment which we made with a frog.

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* Obſervations on Calculus, &c. P. 50.

Having now, in a great meaſure, finiſhed our extracts from Mayow's works, we cannot but expreſs much ſurpriſe, that ſuch noble and confirmed truths, ſhould have lain buried in oblivion for near a century. Every candid reader muſt yield his aſſent, to this very extraordinary anticipation of modern diſcoveries. I feel a degree of veneration for his name, when I reflect upon his genius; and I have derived the ſame ſatisfaction from the peruſal of his works, that was felt by the ingenious Profeſſor of Gottingen. Acutiſſimi ingenii medici Joh. Mayow, ſays Blumenbach, tractatum Oxon. 1674 editum, magnâ cum voluptate legi et relegi.*

The philoſophy of Mayow was publiſhed in an age, remarkable for its production of great literary characters. Contemporary with him were Boyle, Newton, Lower, Willis, and innumerable others, whoſe labours have conſiderably improved the ſciences.† Harvey had but lately diſcovered the circulation; the philoſophic world was engaged in examining the Newtonian and Carteſian doctrines; and the theory of Stahl was juſt adopted by chemiſts, and was in the full career of its glory. Germany was now famous, all over Europe, for its production of able chemiſts; and men were much attached to the chemical labours of that country, which had given birth to Becher, to Van Helmont, and to Stahl. It was impoſſible for a man, who died at the early age of thirty-two, however

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* Inſtit. Phyſiologicæ, P. 114. 1787.

† We may mention Sydenham, Gliſſon, Thruſton, Bartholin, De Graaf, and Stahl.

ever true his theory, or juſt his ſyſtem, to oppoſe, with ſucceſs, ſuch accumulated obſtacles. No champion would be found, hardy enough to ſtem the tide of popular opinion, with theories and experiments not his own; and it is not likely, that many would riſk their literary reputation, in defence of doctrines, which appeared to meet with general diſapprobation. Thus it was, that the opinions of Mayow did not attract notice, till many years after his death, when his works were read and admired by ſome of the firſt philoſophers of the age. His opinions, however, were not generally underſtood, ſo as to form a ſyſtem; the public mind was not ripe for improvement; and thus it was, that his theories were loſt, in the great maſs of diſcordant doctrines. The beſt mode, ſays Dr. Scherer, which led to the perfection of chemiſtry, and which, although now adopted, does not remain entirely exempt from controverſy, was forgotten, and ſuffered to lie in duſt and oblivion, for upwards of a century. It is but a few years ago, that Wiegel, Blumenbach, and latterly, Dr. Metſger, all friends of antient literature, have called the attention of the chemiſt and phyſiologiſt, to the penetrating genius of our author.*

The name of this great man is now reſtored to the annals of ſcience; his genius is acknowledged, his abilities admired, and his opinions confirmed. The unequivocal concurrence of reſpectable teſtimonies, have added luſtre to his literary fame; and may the laurel of merited reputation continually flouriſh, unfaded, which the fatigues of diſcovery have wreathed about his brow.

(A) Ad vitam animalem ritè inſtituendam, imprimis neceſſarium eſt, ut pulmones diatheſi ſanâ potiantur, quo particulæ nitro-aereæ copiâ ſatis amplâ in cruoris maſſam trajiciantur, et cum particulis ejus ſulphureis quoad minima permiſceantur.

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* Theſe phyſiologiſts, however, did not ſufficiently appreciate the merits of Mayow.—Vide Appendix, No. VI. The gentleman who favoured us with this letter, is not the Dr. Scherer who made extracts from Mayow's works. Monſ. de la Metherie, has alſo given a ſhort account of Mayow's chemical opinions, with the plate of his pneumatic apparatus. Vide Obſervations ſur la Phyſique, ſur l'Hiſtoire Naturelle, & ſur les Arts, par M. l'Abbe Rozier. P. 154.

ceantur. Hinc ſi pulmones abſumpti ſint, aut etiam tabidi, flaccidique fuerint, ſanguinis maſſa, ob particularum nitro-aerearum defectum, non ritè fermentatur: unde fit, quod particulæ motivæ utriuſque generis deficiant, eòque ſumma virium dejectio, corporiſque imbecillitas contabeſcentia ſequantur. P. 73.

(B) Si particulæ nitro-aereæ aut copiâ non debitâ in cerebrum deferantur, aut nervos obſtructos pertranſire nequeant, œconomiam animalem et functionem motivam perturbari neceſſe eſt. Atque ab hâc causâ apoplexia, paralyſis, idque genus morbi alii, haud rarò originem trahere videntur. P. 73.

(C) Hinc œconomia animalis valde perturbatur, uti in ebrietate, epilepſiâ, id genus aliiſque morbis, evenire veriſimile eſt: quippe liquores ſulphure volatili referti, veluti ſpiritus vini, oliaque vegetabilium chymica, nimis propere ingeſta, morbos prædictos haud raro inducunt. P. 74.

(D) Huc ſpectat obſervatio, haud ſemel a me facta. Nempe quoſdam noveram paroxyſmis veluti maniacis et inſuper convulſionibus corripi ſolitos, quorum ſputum, dum iſtius modi paroxyſ-

mis

mis laborarunt, igni immiſſum, inſtar olei, ſeu potius pulveris pyrii ritu, in flammam prorumperit. P. 74.

(E) Quoad melancholiam et maniam, probabile eſt particulas ſanguinis ſalino-ſulphureas, in lienis parenchymate detentas, ibidemque cum ſpiritibus nitro-aereis diu efferveſcentes, indolem atribilariam malignamque acquirere. P. 75.

(F) Ad vitam inſuper animalem et functionem motivam inſtituendam requiritur, ut maſſa ſanguinea particulis ſalino-ſulphureis, iiſque debitè evectis impraegnetur, quo viz. particulæ nitro-aereæ iis admiſtæ, efferveſcentiam juſtam in cruoris maſſâ excitent. P. 75.

(G) At vero ſi particulæ ſalino-ſulphureæ, ob fermentationem ſanguinis nimis imminutam non rite exaltentur, aut ob carnis muſculoſæ diatheſin vitiatam non ſatis prompte a cruoris maſſa ſecernantur, functio motiva vix inſtitui poteſt; et utique probabile eſt, laſſitudinem eam ſpontaneam, et ad motum ineptitudinem, quæ ſcorbutum et mortum ictericum comitantur, ab hac causâ procedere. P. 76.

(H) Ex dictis ratio aſſignanda eſt, cur corporis

exercitium

exercitium ad morbum ictericum, ſcorbutumque, item ad febrium intermittentium paroxyſmos præcavendos tantum valet: nempe in exercitiis, particulæ nitro-aereæ majori copia per reſpirationem intenſiorem, in ſanguinis maſſam tranſmittuntur P. 76-7.

(I) Ex quibus ratio petenda eſt, cur ſanguis, in paroxyſmis convulſivis emiſſus, valde craſſus et aliquantulum grumoſus eſſe ſolet: nempe in contractione muſculari violentiori, non tantum particularum nitro-aerearum, ſed etiam ſalino-ſulphurearum (a quibus mutuo æſtuantibus, ſanguinis fermentatio et fluiditas dependent) diſpendia quam maxima fiunt, eoque cruoris maſſam aliquantulum coagulari neceſſe eſt. Hoc etiam tum præſertim contingit, cum etiam partes reſpirationi inſervientes, convulſiones patiuntur, tunc enim ob reſpirationem fere ſuppreſſam, particularum nitro-aerearum jactura in motibus convulſivis facta, non uti alias in motibus violentis per reſpirationem inſtauratur. P. 44.

SECT. XIV.

MAYOW'S WORKS *attended to by various* AUTHORS, *after his* DEATH.

It is a trite, but juſt obſervation, that the works of an author often lie neglected, till the author himſelf has paid the debt of nature. It then not unfrequently happens, that many exert themſelves to bring his works into notice. It requires time, too, to unfold the reaſoning of the mind; and as moſt people have not paid that attention to the ſubject which the author himſelf has, they will not ſo readily underſtand his experiments. As obſervations are made, they confirm the facts, or refute the theory advanced by him, whoſe labours have been over-looked. If the ſubject is of ſufficient importance, at all to engage the attention, the author, in either caſe, is in time taken notice of; and we are often ſurpriſed to find, that truths ſo noble, and the reſult of tedious experiment, ſhould have been neglected, for a conſiderable length of time. This ſeems to have been the caſe with our ingenious author; and it will be a purſuit of pleaſing curioſity, to examine how far his writings were taken notice of, when he was unable to feel the honors paid to his ſuperior genius. The comprehenſive abilities of Mayow, not to be reſtrained within the narrow limits of ordinary minds, graſped, at once, the whole animal œconomy, and endeavoured to explain all its laws, from his admirable diſcoveries. If in the ardour of enquiry, he is ſometimes found to be wrong, we are not to blame the genius of the man, ſo much as the fallibility of humanity. When the mind has been long employed, in inveſtigating the cauſes of natural phænomena, it feels an inexpreſſible pleaſure, in detecting the operations of nature: it gives a ſpur to induſtry, and a reſt for further enquiry. Wholly taken up with the object of its purſuit, and exulting in the ſatisfaction of not being employed in vain, it is apt to ſtray from the principles with which it ſet out, and fall into error, from premature concluſions. It is thus, that many great abilities have been led into miſtakes, by a favourite doctrine.

The opinion, that air held a conſiderable ſhare in the functions of the ſyſtem, had been mentioned, but not underſtood, before Mayow wrote. We may, therefere, expect, that this part of his works would be firſt attended to: accordingly, in publications already examined, ample teſtimony is given, of the confidence placed in the accuracy of Mayow's experiments, and his authority quoted, in confirmation of obſervations on the aerial doctrine. The manner in which the ingenious Wolferſtan explained the effects produced by the bite of the adder, ſhewed, that he had very particularly attended to Mayow's writings. The equivocal expreſſions of, *particulæ nitro-aereæ*, *nitro-ſalinæ*, &c. which Mayow uſed, is a reaſon, why he was ſo eaſily miſunderſtood, by thoſe who did not thoroughly examine into his expreſſions; and alſo afforded an opportunity, for others to aſcribe to him opinions, which he never publicly avowed. Science was not yet ſufficiently improved, to allow of better; and as far as chemiſtry was advanced at this period, no terms could be more aptly applied. Had Mayow's life been ſpared to an older age, his terms would, no doubt, have been altered; as by a variety of experiments, he would have extended his diſcovery of oxygen, more generally, and have ſeen the neceſſity of *inventing* expreſſions, leſs equivocal.

About thirty years after the death of Mayow, a book was publiſhed at Amſterdam, on the ſubject of nitre. Schelhamerus, the author, makes Mayow ſay, that the air is full of nitre, and the nitrous particles is that part of the air, which is ſo neceſſary to the preſervation of life. Another author, of more recent date, attributes the ſame opinion to Mayow. He ſays, that Mayow pretends, ſome ſaltpetre flies about in the atmoſphere*. This is entirely a miſtake, which it is *now*, we hope, unneceſſary to correct. The doctrine, that atmoſperic air was replete with nitrous particles, was not attributed to Mayow by Schelhamerus

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* Tranſlator of Scheele's Eſſay. Should it be aſked, ſays Dr. Beddoes, if the author of this aſſertion had read Mayow, and ſhould it be replied, yes; then let it again be aſked, if he underſtood him; will that queſtion be alſo anſwered in the affirmative?—Letter to Dr. Goodwin, p. 58.

hamerus alone. It was believed to be his, and eagerly adopted by the philoſophers of Europe: and the manner in which Schelhamerus mentions this circumſtance, ſhews ſome jealouſy of Mayow's reputation. Speaking of the abſorption of nitrous particles by the lungs, he adds; (A) Which opinion, *as the preſent age is ſo fond of novelty*, was *greedily* received by the phyſiologiſts of England, France, Germany, and Holland. This is the account which Schelhamerus gives. Are we warranted in ſaying, that, becauſe Schelhamerus thought Mayow mentioned, that the air contained nitrous particles, he attributed the adoption of the ſame opinion to others, who underſtood him in the right ſenſe? Is it not agreeable to think, that of the many who ſubſcribed to his doctrines, ſome muſt have entered into the *true ſpirit* of Mayow's enquiry? Morhof, we ſhall ſee, perfectly underſtood him. Among thoſe, too, who adopted the opinions of Mayow, the celebrated Baglivi of Rome.

The neceſſity of applying a name to *that*, which is abſorbed during reſpiration, muſt have been clearly perceived by all, when that function was better underſtood. The anatomiſt who examined into the ſtructure of the fibre, or the phyſiologiſt who theoriſed upon the nature and effects of reſpiration, muſt have acknowledged, after this period, the efficacy of air, in the functions of the animal frame. The great diſcovery of the circulation had now been made, and acknowledged by all Europe; the full importance of the abſorbent ſyſtem was underſtood; no wonder then, that the beautiful doctrine of Mayow, as it ſo well illuſtrated and *completed* the diſcovery of the circulation, ſhould have gained ſo general a reception in the medical world: and had not the madneſs of politics diſtracted the attention of mankind, at this time, the diſcoveries of Mayow might have led to thoſe brilliant truths, which are now univerſally adopted. It is a remarkable coincidence in the hiſtory of ſcience, that the great diſcoveries in chemiſtry and phyſiology, of the laſt and preſent century, ſhould have been attended with a correſponding revolution in the politics of Europe. The name of Harvey was firſt immortalized amid the broils of civil war, and notwithſtanding Mayow publiſhed his work after the Reſtoration, policits ſtill engroſſed the attention of the public, and the effervescence produced in the minds of men, by the political mania of the times, did not ſubſide, till 1688, when James abdicated the throne of theſe kingdoms. Even after that period, the claims of the Pretender found advocates among many, and politics ſtill intruded upon the peaceful labours of the Muſes. Scarcely had the beautiful ſyſtem of Lavoiſier excited the attention, and claimed the acknowledgment of the chemiſt, when the fury of politics, and the madneſs of war, hurried all things into one undiſtinguiſhed confuſion. How muſt the heart of every man feel, when he reflects upon the murder of that illuſtrious philoſopher. When ſhall we again find combined, his ſpirit, induſtry, and great abilities, which an ample fortune enabled him to employ to their fulleſt extent? It is a loſs, which a century will not repair. Singular infatuation! that even the hoary head of ſcience ſhould be made to bend, without a crime, to the fury of democratic tyranny *. But to return.

Notwith-

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* A great northern philoſopher, whoſe labours have much contributed to our preſent knowledge of the gaſes, in an

Notwithſtanding the exertions of Schelhamerus, to diſcredit the doctrines of Mayow, which he did not underſtand, his truths made their way into the writings of authors of the firſt reſpectability. His aſſertion, that the philoſophers, throughout Europe, attributed to Mayow the doctrine, that nitrous particles are abſorbed by the blood, is remarkably contradicted in the Polyhiſtor. of Morhof. Morhof ſeems to have been better acquainted with the writings of Mayow, than the other authors of his time. He was a man of extenſive reading, accurate judgment, and deep penetration. He travelled to different Univerſities, for the improvement of his mind. (B) He paſt over into England, immediately upon the Reſtoration, and celebrated the inauguration of Charles, at Oxford, in Latin verſes. He alſo reſided at this Univerſity for ſome time,

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an introductory lecture on the Hiſtory of Chemiſtry, when he came to the diſcoveries of Lavoiſier, was unable to proceed; the generous feelings of a humane heart, recurring to the cruel circumſtance of his death, prevented utterance. It is ſaid, that when Lavoiſier was informed, by letter from Dr. Black, of his converſion to the antiphlogiſtic doctrine, ſo great was his joy, in having acquired ſuch able ſupport, that he publiſhed it in all the newſpapers in Paris.

time, to enjoy the benefit of reading, in the extenſive library of the Bodleian. As Morhof made it a point to cultivate the acquaintance of the literary, in the different Univerſities in which he ſtudied, I had imagined, that he might have met with Mayow, when he was at Oxford; but upon adverting to chronology, I find, that was impoſſible, as Morhof was at Oxford in 1660, and, according to Wood, Mayow did not enter the Univerſity, till September 1661, ætat 16.

The aſſiduous application of Mayow, had not only endeavoured to inveſtigate the laws of the animal œconomy, but he employed his diſcoveries to account for various phænomena, which occur in natural philoſophy. He had extended his doctrine to the explanation of combuſtion, reſpiration, the chemical union of ſalts, &c. &c. In moſt of his opinions, he is followed by Morhof, who, on every occaſion, takes an opportunity of paying him a compliment. In various parts of this author's intereſting miſcellany, we find the Tractatus Quinque, as a book of reference. Among other ſubjects, which Mayow had conſidered, that of combuſtion claimed his attention. He imagined, as we have ſeen, that it was owing to the rapid motion of fire-air particles. In this part, he is quoted by Morhof, who ſays, that Mayow's opinion is ſingular; and adds, after having deſcribed his theory,—this, to ſpeak the truth, is very ingeniouſly and curiouſly imagined by him. On the ſubject of nitre, Mayow had very ably employed his genius and his pen; and before his time, the nature of this neutral ſalt was very imperfectly underſtood. The clear and explicit manner, in which he points out it's compoſition, claims our admiration, when we conſider, through what darkneſs and confuſion he had to wade, in his application to this ſubject. The promiſes, which Glauber had made, were not realized, in the ſubſequent pages of his Novum Mundi Miraculum, and his dreams had impoſed both on himſelf and others; neither was any elucidation given to this ſubject, from the expreſs publication of Clarke. After peruſing the treatiſe, De Salnitro, by Mayow, Morhof exclaims; (c) John Mayow has very learnedly, and with much perſeverance, enquired into it's compoſition, in a whole treatiſe upon the ſubject; and has ſhewn, that there is a certain ſpirit, which he terms nitro-aerial, diſperſed through the earth and are, whence air derived the production and fermentation of all things. As Mayow had applied both his attention and experiments, to diſcover the nature and properties of atmoſpheric air, he thought it his buſineſs, to explain the phænomena that are produced by the agency of this great maſs of fluids. The ſingular appearances, cauſed by water-ſpouts, attracted his curioſity, and he determined to exert his abilities, in the explanation of them; and the maſterly manner, in which he has executed his determination, is another laurel, which the hand of Genius has wove into his crown. The vacuum formed by converging winds, did not eſcape his obſervation; and the water riſing on the principle of the Toricellian tube, is the explanation of the accurate Mayow*. Morhof mentions him, as the author who has given the beſt theory of theſe phænomena. As

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* Electricity, certainly, has its influence in the production of water-ſpouts; their ſudden appearance and diſappearance, their occurrence in thunder ſtorms, the flaſhes of light that are ſometimes ſeen iſſuing, the cuſtom that ſailors have, of preſenting pointed ſwords to diſperſe them, all tend to confirm this idea.—Nicholſon's Philoſophy, Vol. II. p. 361.

(D) As to thoſe aerial whirl-pools, termed by the Engliſh, ſpouts, no one has more accurately explained them than John Mayow, who has alſo deſcribed them by plates.

The chemical accuracy of the indeſatigable Mayow, is no where more conſpicuous, than in his examination of Bath-waters. Notwithſtanding his doctrines did not meet with a kind reception, at firſt, yet we learn, from Wood, that he became noted, from his practice in phyſic, and eſpecially in the ſummer time, in the city of Bath. The waters of this celebrated place of faſhionable reſort, would not eſcape the prying eye of one, ſo eager to examine into the operations of nature, as Mayow; and the reſults of his experiments and obſervations, form a curious coincidence with thoſe of Dr. Falconer. It is proper, Mayow obſerves, before we enquire into the cauſe of the heat of theſe waters, to examine their impregnations. The experiments of Mayow, produced the following reſults. The waters become turbid, from the addition of the fixed and volatile alkalis, and they form a curd in boiling milk *. Hence, he concludes, that

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* Falconer on Bath-Waters, Vol. I. p. 240-8.

that an acid is preſent in theſe waters, in a combined ſtate. It appears, from the experiments of Dr. Lucas and Dr. Falconer, that ſelenite, or ſulphat of lime, is contained in them. They are therefore rendered turbid by the precipitation of the lime, the alkali ſeizing upon the ſulphuric acid. According to Mayow, theſe waters contain no nitre, becauſe the reſiduum, after evaporation, when thrown on burning coals, does not deſlagrate like that ſalt *. Dr. Falconer imagined, that the Bath-waters contained ſulphur, in the form of a calcareous hepar. This ingenious gentleman, triturated quicklime and flowers of ſulphur, with diſtilled water, and by adding the different alkalis, in their mild and cauſtic ſtate, obſerved, that the ſame appearances were produced as in the Bath-waters. An objection has been made to this experiment, by a writer on the materia medica, who, had he exerted his chemical abilities, would have diſcovered, that this objection was founded in error. He maintains, that ſulphur and quicklime, triturated with cold water, produce a ſimple lime-water, without ſulphur. This is a miſtake; for the

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* Vide Falconer, p. 239.

the heat generated during the union of the quicklime and water, is ſufficient to produce a ſolution of ſome of the ſulphur. Dr. Lucas, however, maintains, that they contain no ſulphur; Mayow alſo thinks ſo, and gives a curious reaſon, why it was imagined they contained ſulphur. It is a prevailing opinion, he ſays, that ſilver is tinged yellow by theſe waters; but this is by no means the caſe, for the deception has ariſen in the following manner. It is a cuſtom with the keepers of the baths, to colour pieces of money with the ſulphureous filth of privies, and then ſell them to ſtrangers for a moderate compenſation. According to our author, theſe waters contain no ſal ammoniac; and the chalybeate impregnation, which they are ſaid to poſſeſs, was detected by him. (F) The Croſs and Hot-Baths, he ſays, contain no iron; but the King's Bath ſtrikes a black colour, with infuſion of galls. This, too, accords, in ſome meaſure, with the experiments of Dr. Falconer, who ſays, that they all ſhew the preſence of iron; but, beyond all doubt, the chalybeate impregnation is moſt perceivable in the King's Bath water*, and Dr. Lucas detected moſt iron in the water from the ſame Bath.

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* P. 232.

Mayow mentions a very curious circumſtance, unnoticed by other authors; and as he is very accurate in other reſpects, I cannot doubt it. (G) He ſays, that the ſabulous matter, which riſes with the water from the ſpring, ſhews no impregnation of iron, upon the addition of infuſion of galls; but if the vitriolic acid be added, a briſk effervefcence takes place, and then the infuſion will ſtrike a black colour. Hence, he concludes, that this ſand contains a metallic impregnation, convertible into a vitriol, by the addition of the acid. It appears, at firſt ſight, a remarkable circumſtance, that authors ſhould have varied ſo much, in their analyſis of mineral waters; but when we conſider the fallacy that often obtains in theſe nice experiments, and alſo the variety of minutiæ, which eſcape the attention, we muſt be ſatisfied, with the imperfect knowledge of their impregnations. It appears to me highly probable, that from the continual decompoſitions which nature is undergoing, the quantities and qualities of the different impregnations muſt vary, at different times, producing no very ſenſible alteration, except by the nice analyſis of chemical teſts. Hence, probably, the reaſon, why the reſults have ſo often varied. I think it rational, alſo, to conclude, that they are continually undergoing a change, and repeatedly return to the ſame qualities, by ſucceeding recompoſitions, from various cauſes, which we daily ſee done artificially by the hands of the chemiſt. The coincidence between the experiments and obſervations of Mayow, and thoſe of Dr. Lucas and Falconer, is curious and remarkable. In him were combined the rare qualities of induſtry, genius, and perſeverance. Undiſmayed by the difficulties of purſuit, his ardent mind, with a perception attendant only on great abilities, examined into the ſecret operations of nature. His clear explanation of complicated experiments, his indefatigable induſtry, and conſequent diſcoveries, have handed down to poſterity, the name of Mayow. It was impoſſible for a mind, ſo vigorous as his, to reſt ſatisfied, with giving a ſimple explanation of the contents of Bath-waters; their heat, alſo, arreſted his attention, and he was determined to explain the cauſe. It was an opinion, pretty generally adopted, in the time of Mayow, that the heat of ſprings was owing to ſubterraneous fire. Dr. Jordan was one of the firſt who rejected this doctrine, and maintained, that from fermentation, was derived the cauſe of this never-failing heat. It was not ſufficient for Mayow to know, that fermentation produced this phænomenon; the cauſe of *that*, alſo, muſt come under his examination. It is owing, therefore, to the decompoſition of martial pyrites. The ſulphuret of iron, being moiſtened, becomes heated, and Mayow ſays, this is produced by the fermentation, which takes place between the air of the water, and the ſulphureous part of the marchaſite. Will the martial pyrites, moiſtened *in vacuo*, produce heat? Is it not owing, according to Mayow, to the decompoſition of atmoſpheric air? It is a well known fact, that iron and ſulphur, kneaded together, with a little water, will abſorb oxygen from the atmoſphere. That water contains air, Mayow ſays, is proved, from the bubbles which are emitted, during the exhauſtion of an air pump. In further confirmation of this, he relates an experiment, which deſerves a deſcription. A veſſel, filled with water, is immerſed, with its mouth downwards, into a larger one, containing the ſame fluid. The water is made to boil, and afterwards ſet by to cool: we ſhall then find a quantity of air, collected in the upper part of the inverted veſſel; and this he found, by his experiments, to poſſeſs the elaſticity of common air. In peruſing many paſſages of this moſt ingenious author, my mind is ſometimes unwilling to believe, that they were publiſhed as far back as 1688; ſo aſtoniſhingly ſimilar are they, to the chemical doctrines, and mode of experiment, lately promulgated; ſo ſtrikingly great is the analogy between them. That air, too, is contained in water, is evident, from the life of marine animals; for fiſh ſoon die in water, from which the air has been exhauſted. (H) In this caſe, they turn on their backs; becauſe the air in their air-bladder exerts its elaſticity, and the abdomen is diſtended, the atmoſpheric preſſure being removed. He will not determine, whether the contents of the air-bladder enter the maſs of blood, as in reſpiration, but he thinks it

probable;

probable; becauſe fiſhes will live longer than other animals, in an exhauſted receiver.*

So juſt and able account of the heat of mineral waters, would not fail to acquire the aſſent of the accurate Morhof: no wonder, then, when he found Mayow ſo clear in his ideas, and ſo well arranged in his facts, that he rejects the doctrines of others, and prefers Mayow's in this, as in other inſtances. (l) More agreeable with truth, appears to be the opinion of John Mayow, who following Jordan, imagines the heat of mineral waters to ariſe from fermentation, produced in the bowels of the earth; *viz.* the particles of air deſcending into the earth, with the rains, and there meeting the ſalino-ſulphureous mineral, produce heat in it, and the ſprings ariſing from the mineral, thus heated, conſtitute the warm mineral waters. The ingenious

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[*] My very worthy friend, Dr. Brodbelt, of Jamaica, has diſcovered, that the air, in the air-bladder of the ſword-fiſh, is pure oxygen, and which, he very juſtly obſerves, is to ſerve the purpoſes of life, when the animal is far below the ſurface of the water.—Vide Letter to Dr. Duncan, in Med. Annals.

ingenious author of the Polyhiſtor. takes notice of Mayow, on ſeveral other occaſions. The abilities he had diſplayed, in his treatiſe on reſpiration, did not eſcape the attention of Morhof, who recommends his work, on this ſubject, with thoſe of Schwammerdam and Thruſton. It is evident, from what has been detailed, that Morhof ſet a value on the labours of Mayow; whether, however, he clearly perceived what Mayow meant, by his nitro-aerial particles, may perhaps be doubted. If we are to judge, merely, from the manner in which he quotes Mayow, it would appear, that he entered into the ſpirit of his author; on the contrary, when we find no explanatory account of theſe quotations, a ſuſpicion may be entertained, of his complete knowledge of their meaning*.

It requires more than a curſory reading, to under-

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* The authority of Mayow, as a writer of credit and reſpectability, was referred to and relied upon, in other branches, beſides that of chemiſtry. Hanſen, in his Inaugural Diſſertation on Rickets, publiſhed at Gottingen, 1762, quotes him in the honourable company of Gliſſon, Petit, Heiſter, Boerhaave, Hoffman, Sydenham, &c. and he is alſo frequently mentioned, by the commentatro on Van Horne's Microcoſ.

underſtand his works, eſpecially when we conſider, that at this period, the language of the chemiſt was not altogether free from the jargon of alchemy. As ſoon, however, as the compoſition of the atmoſphere, and the knowledge of the different gaſes began to be developed, by the labours of Hales, of Prieſtley, and of Black, Mayow's works immediately arreſted the attention of the philoſophic world. In confirmation of the truth of this obſervation, we need only remark, that almoſt directly upon the ſuppoſed diſcovery of fire-air, by Prieſtley and Scheele, a celebrated writer on phyſiology immediately referred to Mayow, as the original diſcoverer *.

It is well known, with what enthuſiaſm Dr. Beddoes publiſhed his extracts from the works of Mayow. The conciſe manner, in which he treated the ſubject, barely did juſtice to the genius of the author. If we have at all filled up the deficiency, or, in any wiſe, impreſt the chemiſt and phyſiologiſt, with the reſpect due to the abilities of ſo great a man, we ſhall feel the moſt ſenſible ſatisfaction; and happy are we, from the inveſtigation we have gone

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* Blumenbach.

gone through, in being able to contradict the too haſty concluſion, that his name, as it never was echoed by popular applauſe, was ſoon forgotten among men, and his memory obliterated.

(A) Quæ ſententia, ut eſt novitatum amans hoc ſeculum, ſtatim ab omnibus, vel tantum non omnibus Anglis, Batavis, Gallis, Germanis, avidè arrepta eſt. P. 100 *.

(B) Exeunte eodem, A. 1660, in Bataviam et Angliam abiit, viros utriuſque celebres ſalutavit, Caroli II, Britanniæ regis, inaugurationi interfuit, ſuumque applauſum carmine illi teſtatus eſt gratulatorio, et in academia Oxonienſi, ut bibliotheca hujus illuſtri uteretur, aliquandiu ſubſtitit. Prolegom. P. 10 †.

(C) Longe

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* Guntheri Chriſtoph. Schelhameri, de Nitro Commentatio. Amſtelodami, 1709.

† Danielis Georgi Morhofi, Polyhiſtor. Literarius, Philoſophicus, et Practicus, Edit. à Johanne Mollero, Flenſb, Lubecæ, 1714.

(C) Longe doctius and magno molimine, nuper in ejus materiam inquiſivit toto libro Johannes Mayow; qui oſtendit, per totam terram et aerem ſpiritum quendam, quem nitro-aereum vocat, eſſe diſperſum, unde omnium rerum fermentationes et germinationes deducit. Lib. II. Part 2, Cap. 38.

(D) Quod ad vortices illos aereos attinet, qui Anglis *ſpout* dicuntur, eos nemo accuratius, iconibus etiam additis deſcripſit, quam Joh. Mayow. Lib. II. Part. 2. Cap. 23.

(E) Balneorum cuſtodibus familiare ſit, nummos argenteos fimo ſalino ſulphureo, qualis in cloacis paſſim reperire eſt, tingere et veluti deaurare, dein eoſdem, tanquam aquis balneorum tinctos, peregrinis pro quæſtu modico venditare. P. 251.

(F) Quod ad vitriolum denique ſpectát, balneum, vulgo dictum Balneum Crucis, item alterum Præfervidum nominatum, vitriolum plane nullum continere videntur; etenim ſi gallæ contuſæ aquis thermarum dictarum infuſæ fuerint, aquæ iſtæ colorem purpureum aut nigrum nequaquam habituræ ſunt. Quod ad Balneum Regis, iſtoc vitrioli tantillo impraegnari videtur, quippe ſi gallæ contuſæ, ejus aquæ injiciantur, eodem colore atro-purpureo leviter tingetur. P. 151-2.

(G) Si ſabulum iſtoc balneorum liquore acido impregnatum, infuſioni gallarum injiciatur, liquor mox colorem atro-purpureum acquiret; cum tamen, ei infuſio gallarum ſabulo iſti recens a balneis exempto affundatur, ea nequaquam colorem purpureum obtinebit. P. 252.

(H) Illud hic loci obiter annotare liceat, quod ſi piſcis aquæ in vaſe idoneo contentæ impoſitus, intra vitrum includatur, ex quo aer exhauritur, idem non ut antea pronus, ſed e contra ſupinus natabit; quin et venter ejuſdem ex parte aliquâ ſupra aquæ ſuperficiem elevabitur, quorum ratio eſſe videtur, quod aer in veſicâ natatoria incluſus, quamprimum aeris externi preſſura aufertur, ob vim, quâ pollet, elaſticam ſe ſtatim extendit, ita ut veſica prædicta, uti etiam cavitas abdominis ab aere iſto inflentur. P. 259-60.

(I) Rectior eſſe videtur ſententia Johannis Mayowii, qui Jordanum, popularem ſuum, ſecutus, thermarum incaleſcentiam a fermentatione, in terræ viſceribus alicubi excitata, provenire exiſtimat, ſic, ut particulæ aereæ, una cum aquis

pluvia-

pluvialibus, in altam tellurem deſcendentes, ibidem mineræ ſalino-ſulphureæ occurrentes, æſtum admodum intenſum in eadem excitent, et demum aquarum ſcaturigines e minera, eo modo exæſtuante, profluentes, thermas conſtituunt. Lib. II. Part. 2. Cap. 20. Mayow. 261-2.

CONCLUSION *to the* FIRST PART.

Having ſhewn, how accurately different authors were acquainted with the operation of air upon our bodies, during the laſt century, we preſume it will form no unintereſting part of the preſent work, to trace the progreſs of opinion, on this ſubject, from that period to the preſent times. We have ſeen, how extenſively diffuſed Mayow's doctrine was, although greatly miſunderſtood by ſome phyſiologiſts. We are, therefore, not ſurpriſed, to find his expreſſions adopted, without knowing to whom they belong, and his doctrine affecting the arguments of the phyſiologiſt, to the diſcoveries of Prieſtley and others. Certain it is, that almoſt immediately after the time of this moſt ingenious phyſician, we find, in a variety of authors, the reaſoning of the chemico-phyſiologiſt more clear, diſtinct, and conſiſtent with the truth; but occaſionally, as is natural, we diſcover deviations from the ſpirit and meaning of the original diſcoverer. There is a tide in diſcoveries, which, like a ſtream, flows neglected and unperceived from its ſource, and occaſionally is encreaſed or diminiſhed, in proportion as it is confined, or receives it tributary waters, till at laſt it burſts into a broad and extenſive plain: it then arreſts the attention, and obliges us, by its magitude, to reflect on its ſource.

When it was known, that neither fire nor life could be ſupported, without the preſence of air, it was natural, from analogy, to transfer the term, vital flame, to animal exiſtence; it was a juſt inference, too, to ſay, that this flame reſided in the air. That this was actually the caſe, we find, in various publications, ſince the time of Mayow; and which we ſhall now examine. Fire, flame, or heat, abſtractedly conſidered, was regarded as the grand ſtimulus to all the motions of the animated frame; it was the vital flame, reſiding in the blood*. No clear diſtinction, however, was made between the matter of heat and fire, or flame. The want of this diſcrimination, tended very much to confuſe the ideas and perplex the reaſonings of [illegible] the phyſiologiſt, till the beautiful theory of Dr. Black was publiſhed. The air, ſays Ray, ſerves us and all animals, to breathe in, containing the fewel of that vital flame we ſpeak of, without which it would ſpeedily languiſh, and go out*. This general proportion, involves in it facts of great extent and importance. We all know, how near it is to the truth; yet the nature and qualities of that fluid, which ſupplied the vital flame, were as unknown to Ray, as to the predeceſſors of Mayow. He ſaw, however, that ſomething different from common air, but contained in it, ſupported life; but its properties he found it difficult to underſtand. The neceſſity of an aerial pabulum was felt, and ackowledged by him; for he maintains, that the blood, in its paſſage through the lungs, receives air, as the fewel of the vital flame, and thus impregnated, returns to the heart†. Animal heat was the effect, too, of this acquiſition of air: hence animals of hot blood have two ventricles, by which that fluid has a more frequent communication with the air in the lungs, ſo requiſite to muſcular motion.

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* The Wiſdom of God, manifeſted in the Works of the Creation, by John Ray, F. R. S. London. 1692. P. 61.

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* P. 62. † P. 48.

tion*. The idea of animal heat being produced from the air, imbibed by the lungs, occurred to Ray, from the conſideration that horſes, ſheep, &c. remained with impunity in the open air, during the coldeſt weather. Conſidering with myſelf, by what means they were enabled to do this, and to abide and reſiſt the cold, it occurred to me, that the extremities of their toes were fenced with hoofs, which in good meaſurue ſecured them; but the *main thing* was, that the cold is, as it were, its own antidote; for the air being fully charged and ſated with nitrous particles (which are the great efficients of cold, and no leſs alſo the pabulum of fire) when inſpired, doth by means of them, cauſe a great accenſion and heat in the blood (as we ſee fewel burns raſhly in ſuch weather) and ſo enable it to reſiſt the impreſſions of the cold, for ſo ſhort a time as its more nimble circulation expoſes it thereto, before it comes to another heating *.

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* P. 100. Did Ray ever conſult the works of Mayow? He no where quotes him; but we ſuppoſe had an *inkling* of his writings, from the following expreſſions. When ſpeaking of the damps in mines, he ſays, indeed though there were no damps, yet the *nitrous part* of the air being ſpent, and conſumed by the breathing of the miners, the remaining part would be rendered altogether unfit for reſpiration, unleſs new and freſh air could ſucceed. P. 69-70.

The apparent want of air to the fœtus in utero, has often been the grand objection urged by thoſe, who maintained the non-abſorption of air by the blood in the lungs. This formed no obſtacle to Ray, who explains it in a manner ſimilar to Mayow; for he ſays, that the fœtus does receive as much as is neceſſary for it, in its preſent ſtate, from the natural blood, by the placenta uterina or cotyledons. That the reſpiration of the dam did ſerve the fœtus alſo, or ſupply ſufficient air to it, I have met with in books; but the explicit notion of it, I owe to my learned and worthy friend, Dr. Edward Hulſe †. It was a doctine then more prevalent, at that time, than we

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* P. 102. Here, by not making a proper diſtinction, he falls into error, which would not have happened, had he read and underſtood Mayow. From him he would have learnt, that it is only one of the principles of nitre, which by decompoſition produces heat; and that the neutral ſalt, only during ſolution, produces cold.

† P. 65.

we are apt to imagine, but which owed its origin to Everard, and was more clearly elucidated and improved, by the experiments and obſervations of Mayow. The chief uſe, therefore, ſays Ray, of the circulation of the blood, through the placenta uterina, ſeems to be the impregnation of the blood with air.

When the principles of a ſcience have been developed, and have rendered familiar and eaſy the explanation of phænomena, we are, in ſome degree, aſtoniſhed to find, in older writers, facts laid down, and partly explained, but which required a much greater improvement, in that branch of knowledge, perfectly to underſtand. No one will be bold enough to aſſert, that Ray was acquainted with that principle, we now call oxygen; yet it is evident to conviction, that he knew there was an abſorption of a vivifying quality from the atmoſphere, and which was ſupplied to the fœtus, through the medium of the mother. He not only underſtood, that ſomething was imbibed by the lungs, but maintained, that it was the ſupport of the vital energy. This, ſurely, was a great point gained; and the doctrine would have received more firm ſupport, and would have been more eagerly adopted, had Mayow's writings been thoroughly examined, and more generally underſtood. The fluctuation of opinion, with reſpect to a particular doctrine, ariſes, frequently, as much from the want of a general knowledge of the ſubject, as from a prejudiced attachment to a favorite theory. In examining into the works of ſucceeding authors, we are ſurpriſed to find one, who ranks high in the ſcale of the medical world, totally unacquainted with the nature of reſpiration, although this function had been examined, and its object pointed out, in the moſt unequivocal manner, by men of the firſt literary abilities*. It is a curious circumſtance, that notwithſtanding Liſter did not underſtand this important function, yet he diſtinguiſhes between atmoſpheric and vital air; but in a manner which confounded, inſtead of elucidating the ſubject. He maintained, that notwithſtanding air was neceſſary to life, yet it did not unite with the blood, in the lungs, but entered with the chyle†. There is a difference, he ſays, between pure air or æther, and common or atmoſpheric air : the former is, every where, clear, pellucid, inviſible, and poſſeſſed of a certain degree of elaſticity; the latter is compoſed of two kinds of *halitus*, and contains fire and water. The one *halitus* is moiſt, and evidently the vapour of water, produced by evaporation; the other is a warm and dry ſpirit. Of theſe two kinds of *halitus*, therefore, is compoſed atmoſpheric air, which, on that account, is to be conſidered both as dry and moiſt. Æther, therefore, or pure air, by which animals are ſurrounded, being replete with a dry and aqueous vapour, conſtitutes vital air *. This vital air, deſcending into the ſtomach with our maſticated food, is the cauſe of digeſtion; and its ſulphureous part, after it enters the lacteals, produces animal heat, by agitation with the chyle in the heart. The following is a remarkable obſervation, which he makes, with reſpect to animal heat; and he would have been able to have explained the fact, had he underſtood the nature of reſpiration. Our food, he ſays, is either a cauſe of animal heat, or an indiſpenſable condition to its generation †. A cer-

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* Lower, Mayow, Thurſton, &c. &c.

† Diſſertatio de Humoribus, P. 29-30.

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* P. 88. † P. 97.

A certain ſact, which the chemical doctrines have beautifully illuſtrated. I feel myſelf totally inadequate, to give any clear account of the diſtinction which Liſter has made, between atmoſpheric and vital air. The confuſed manner in which he has handled this part of his work, ſhews, that he himſelf poſſeſſed no diſtinct ideas of the ſubject. He ſeems to have taken his notions from the doctrine of Hippocrates; but rendered more confuſed, by adopting the language of the Pythagorean School. He applied the term vital, to that part of the air, which Hippocrates called *ſpiritus*, and which was neceſſary to the nouriſhment of animals. I ſhould have deemed it unneceſſary to have mentioned this work, did I not wiſh to point out, that although certain laws of the animal œconomy could not be underſtood, without having recourſe to the air, yet that ſo remarkable a deviation ſhould be made from the more ſimple and obvious explanation of Mayow, Ray, and others.

Soon after the commencement of the eighteenth century, many ingenious men inveſtigated the nature and properties of air, with its effects on the human frame. The labours of Hales, of Arbuthnot, and of Bryan Robinſon, principally claim our attention, till we arrive to within a very few years of the late chemical diſcoveries, when our admiration is due to the genius of a great phyſician, the late celebrated Dr. Hugh Smith. The laborious experiments of Hales, furniſhed ample matter for future inveſtigation, by ſhewing how much might be done. The *quantity* of air, which he diſengaged from a variety of ſubſtances, ſeemed to take up his attention, much more than their properties. No juſt or uſeful diſcrimination was made, between the various airs which eſcaped, during his chemical operations. They all appeared to him to poſſeſs nearly the ſame quality; and when they were rendered unfit for reſpiration or combuſtion, it was owing to the loſs of their elaſticity*. The term, loſs of elaſticity, has been uſed by Hales and others, to expreſs that diminution, which takes place in the bulk of air, from reſpiration and combuſtion; not knowing, that every kind of air, when heated, is capable of nearly the ſame degree of expanſion. Hales imagined, that elaſticity was a quality, of which the air might, *in toto*, be deprived. I made ſome attempts, both by fire, and alſo by fermenting and abſorbing mixtures, to try, if I could deprive all the particles of any quantity of elaſtic air, of their elaſticity; but I could not effect it*. Owing to this idea of elaſticity, Hales maintained, that combuſtion was not maintained, nor animal life ſupported, by the vivifying ſpirit of air. The candles and matches ceaſing to burn, ſoon after they are confined in a ſmall quantity of air, ſeems not to be owing to their having rendered that air effete, by having conſumed its vivifying ſpirit; but ſhould rather be owing to the great quantity of acid fuliginous vapours, with which that air is charged, which deſtroy a good deal of its elaſticity, and very much clog and retard the elaſtic motion of the remainder †.

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* Statical Eſſays, Vol. I. P. 258-75.

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* P. 312. The term elaſtic, is very improperly applied, in theſe caſes; becauſe it is owing, altogether, to the interpoſition of another matter, *viz.* caloric; and the degree of expanſion will be, in proportion to the quantity of caloric applied: for the ſame reaſon, iron, when heated, may be ſaid to be elaſtic.

† P. 275. A very conſiderable ſtep was made, by Hales, in improving our knowledge of the airs. He acciden-

As a diſciple of the Boerhaavean ſchool, Hales attributed animal heat to friction; and maintained, that the blood was cooled, in its paſſage through the lungs. He quotes Mayow in two places, yet pays no attention to his diſcovery of pure air, and his application of it to the animal œconomy [*]. There is a very particular experiment in the Statical Eſſays, extremely ſimilar to one of Mayow's, which proves, that air, once breathed, is unfit for reſpiration. The coincidence is ſo ſtriking, that I could wiſh to quote the paſſages, were they not too long [†]. Dr. Hales found, that he could breathe air, through cloths ſprinkled with vinegar, as long again, as the ſame quantity of air not thus managed [‡]. I could point out, were it

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tally hit upon an experiment, by which he produced nitrous air. This he obſerved to form red fumes with atmoſpheric air, with a diminution of its bulk. He procured it from Walton pyrites, iron-filings, and mercury, with nitrous acid. We cannot call it his diſcovery, as Mayow had already done the ſame; and it is ſurpriſing, that he ſhould not take notice of this circumſtance in that ingenious author.

[*] P. 234-36.

[†] Compare P. 98-299 of Mayow, with Stat. Eſſ. P. 169-255.

[‡] For the Note, Vol. I. P. 266.—Vol. II. P. 321.

neceſſary, in ſeveral parts of the Statical Eſſays, expreſſions and obſervations, preciſely ſimilar to thoſe of Mayow, as the fermentation of pure air with ſulphureous particles, &c. I am ſure it is unneceſſary for me to mention, that I have not the ſmalleſt ſuſpicion of plagiariſm by that admirable philoſopher; a character which all reſpect and admire. I only wiſh it to be obſerved, that had Dr. Hales applied his abilities, with proper attention, to Mayow, he would have been leſs perplexed, in accounting for the multiplied variety of his experiments, and thus, as it were, by beginning where Mayow finiſhed, he would have conſiderably advanced the progreſs of knowledge. Had he, too, properly weighed the importance of Mayow's diſcoveries, would he then have broke out into the following exclamation? ‘If thoſe who unhappily ſpent their time and ſubſtance, in ſearching after an imaginary production, that was to reduce all things to gold, had, inſtead of that fruitleſs purſuit, beſtowed their labour in ſearching after this much neglected volatile Hermes, who has ſo often eſcaped through their burſt receivers, in the diſguiſe of a ſubtile ſpirit, a mere flatulent exploſive matter, they would then, inſtead of reaping vanity, have found their reſearches rewarded, with very conſiderable and uſeful diſcoveries.'* The varied and multiplied experiments of Hales, inſtead of deterring, ſeemed to ſtimulate others to undertake the ſame ſubject. The great quantity of air diſcovered by him, in almoſt innumerable ſubſtances, obviouſly pointed out, that its preſence was intended to anſwer ſome uſeful purpoſe. The alteration produced in the function of reſpiration, by different ſtates of the air, ſhewed, that it had it's effects upon the œconomy. The various phænomena that occurred from modified airs,† but above all the apparent connection between epidemical diſeaſes and a ſtate of the atmoſphere, pointed out this ſubject, as one very fit for the examination of the phyſician and phyſiologiſt. Accordingly, it attracted and merited the attention of Dr. Arbuthnot, who, in 1733, publiſhed an Eſſay on the Effects of Air on Human Bodies; a work replete with judicious and accurate remarks. His obſervations are a collection of facts, which experience had ſuggeſted, but which he could not explain, for the want of chemical knowledge. He knew, that a portion of air, once breathed, was rendered deleterious to animal life; this he learnt from Hales; but he was entirely unacquainted with the cauſe. He was led into error, by attending too minutely to Hales's experiments, which did not diſcriminate between the effects produced by different kinds of air. After the manner of that philoſopher, Arbuthnot attributed the effects, produced by reſpiration and combuſtion, to the action and re-action of elaſtic air-particles. He, however, was obliged to acknowledge, that air had many uſes and effects, in the animal œconomy, beſides what was deducible from mechanical principles; and that there is in it ſome other vital principle, which makes it ſo neceſſary for the life of all animals*. We come now to an author, whoſe genius and abilities have conſiderably improved the ſcience of medicine. The name of Bryan Robinſon, is well known in the medical circles; and his Treatiſe on the Animal

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* Vol. 1. P. 317.

† By modified air is meant, an alteration produced by impregnation with different matters, as may be ſeen in the experiments of Hales, and others, previous to this time.

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* P. 115.

mal Œconomy, will not detract from his merit by being examined. In this work we find, the mind returning into the path of truth, after it had been mutilated, for upwards of fifty years, by the crude ſpeculations of Liſter, and the unconnected experiments of Hales *.

There is nothing which ſo much retards the progreſs of knowledge, as the neglect of the labour of our predeceſſors. Harvey would, probably, never have diſcovered the circulation, had he not made himſelf acquainted with the knowledge of the antients; and it is not improbable, that the writings of Hales, and others, led Prieſtley and Cavendiſh to their diſcoveries. The mind of an individual is not ſufficiently vigorous to inveſtigate, in its fulleſt extent the minutiæ of ſcience; ſomething muſt be previouſly and repeatedly performed, otherwiſe *all* the knowledge, which we now poſſeſs, would have preſented itſelf to the experiments and obſervations of philoſophers, many centuries ago. A lucky train of thought, an accidental experiment, or a caſual explanation, have led to ſome of the moſt brilliant diſcoveries, which adorn and enrich the different ſciences. Chemiſtry and phyſiology, had received conſiderable and important improvements from the labours of Mayow, Lower, Wolferſtan, and others; but from neglect and inattention, theſe noble truths were buried in oblivion. Their merit, indeed, had acquired votaries, both at home and abroad; but from ſome unfortunate circumſtances, their diſcoveries were not followed up, which would have prevented that confuſion, which has crept into the experiments and ſpeculations of ſucceeding chemiſts and phyſiologiſts. After peruſing the writings of Mayow, Lower, Gliſſon, &c. what a great falling off do we obſerve, when our reading leads us to the publications of Boerhaave, Hales, and innumerable others. Notwithſtanding this, however, ſcience is indebted to the latter, for conſiderable improvements; they ſet that inveſtigation on foot, which had been neglected, in the writing of their predeceſſors, and which, but for their exertions, would have been longer conſigned to accompany muſty records, in the duſty corner of antiquated libraries.

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* We hope the reader does not imagine, that we mean to depreciate the merit of this ingenious experimentaliſt. The manner in which the ſubject was treated, could not have afforded any other reſult. We are rather ſurpriſed, however, that he did not attempt to make any diſcrimination, between the variety of airs which eſcaped, during his chemical operations.

It is with pleaſure we peruſe the works of Bryan Robinſon, who, by going ſyſtematically to work, and arranging accurately his facts, has ſtruck out ſome of thoſe truths, which are now generally received. The following is the poſition he lays down to be proved. The life of animals is preſerved, by acid parts of the air, mixing with the blood in the lungs; which parts diſſolve, or attenuate the blood, and preſerve its heat, and by both theſe, keep up the motion of the heart *. This, he proves, in the following manner: Animals die, when they are deprived of air; and they alſo die ſoon, in a ſmall quantity of air, cloſely confined. Under ſimilar circumſtances, a candle goes out; glowing coals, and red-hot iron, ceaſe to ſhine. Animals, alſo, die in air, rendered effete, by burning coals or candles in it till they are extinguiſhed; and *vice verſâ.* If air be rendered effete, or blown on live coals, it puts out the fire; but if freſh air be admitted, the coals begin to burn, and ſhine afreſh: and he adds this remarkable fact, that common air, by being paſſed through red-hot iron, red-hot charcoal, or the flame of ſpirit of wine, becomes unfit to preſerve life, and the ſhining of fire or flame. From theſe unequivocal and well authenticated facts, he could not but conclude, that the very ſame parts of the air, which preſerved animal life, by reſpiration, alſo maintained flame, during combuſtion. He concluded, that this part of the air was acid, from the well known experiment of mixing nitrous acid with unctuous ſubſtances. He performed Mayow's experiment, of burning ſulphur and nitre, under water, without acknowledging the author; and tranſlates pages from Lower, concerning the red colour which the blood acquires in the lungs, but does not mention his name. He takes conſiderable pains to prove, that it is not the elaſticity of the air, which preſerves life and flame; becauſe the air which remains, after a candle is extinguiſhed, or an animal dies, is itſelf elaſtic. The air is, alſo, the cauſe of animal heat; for ſpirit of vitriol, which contains acid air, diſſolves iron-filings, with great heat and ebullition; and the acid of the air, diſſolves ſulphureous and unctuous ſubſtances, with the heat of fire and flame. Hence, we may conclude, that ſince the acid of the air unites with the blood in the lungs, and attenuates it, the ſame effect muſt be produced there *. That the motion of the heart is owing to the ſame cauſe, is evident; becauſe, if the acid air be with-held from reſpiration, the pulſe becomes ſmall and quick. The ſame reaſon, which induced Mayow to give the name of fire-air particles to oxygen, alſo perſuaded Robinſon to call that gas, acid air; and from a ſimilar cauſe, have the more recent diſcoverers given it its preſent appellation. The moſt rigid ſceptic could not have with-held his aſſent, to the cloſe reaſoning of Robinſon; for when chemiſtry was not yet analyſed into its elementary principles, his obſervations were perfectly conſiſtent with truth. Dr. Robinſon ſaw

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* P. 190.

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* By a ſubſtance being attenuated, I underſtand its being rendered more fluid. If a ſubſtance, we ſhall ſay, be compoſed of two kinds of particles, the ſimple interpoſition of a third, muſt make an alteration in the ſtate of agregation, *i. e.* remove the original particles at a greater diſtance from each other, or render the ſubſtance more fluid. A chemical interpoſition would produce different effects, according to the degrees of affinity between the elementary principles.

ſaw the air united with the blood in the lungs; the ſame he knew to be a conſtituent principle of the acids; and, as the acids produce heat, in diſſolving various ſubſtances, it was natural to conclude, that when the acid air came into contact with the blood in the lungs, it cauſed animal heat in the ſame way. The genius of Black had not yet illuſtrated, by his beautiful doctrine of latent heat, ſome of the moſt important chemical phænomena; neither was it ſtrictly aſcertained, that atmoſperic air (for Mayow's experiments were not ſtudied) could be analyſed into two component parts *. The concluſions of Robinſon were, therefore, perfectly juſt; and if the reader will take the trouble of examining Prop. 24, from P. 190 to P. 206, in his treatiſe on the animal œconomy, he will find, with what a remarkable accuracy of obſervation his experiments and facts were illuſtrated.

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* Pure atmoſpheric air is compoſed of twenty-eight parts of oxygen, to ſeventy-two of azot; the other gaſes, which are found in it, are accidental, and vary according to local circumſtances.

The period was now faſt approaching, when the whole doctrine of the humoral pathology began to be ſuſpected; for experiment and obſervation now taught, that neither viſcid lentor, nor error loci, could, in every caſe, be the cauſe of fever or inflammation. The doctrines of Hoffman had made their way into the hands of the ſtudents of Edinburgh, and found a more ready reception than thoſe of Boerhaave; at this time, the humoral pathology was at its acmè, being taught by the profeſſors, who had been all pupils of Boerhaave.

The ingenious Dr. Gregory, of Edinburgh, has the following obſervations: I conſider Hoffman, as the original and great author of the revolutions in the theory of medicine, for theſe laſt fifty years paſt. The Boerhaavean doctrine was then taught, in this place, by the profeſſors, who had all been the pupils of Boerhaave; and it is curious, that the revolution was firſt began here by the ſtudents, ſome of them having got hold of the works of Hoffman. The Boerhaavean doctrine was firſt overthrown, in the Medical Society, fifty years ago; this I have, upon the authority of my father, who was a member of that ſociety *.

ſociety*. When the mind has once perceived error, in a particular ſyſtem, it becomes ardent in the purſuit of fallacy; for ſomething is then promiſed to the fatigues of labour and inveſtigation. The doctrine of the importance of air, in the functions of the animal œconomy, was gradually ſtealing upon the minds of men, at this time, and in every part of the world it ſeemed to engroſs the general attention†. In proportion, therefore, as the ſuſpicion of the truth of former ſyſtems increaſed, ſo would a greater importance be attached to the doctrine of air, now become more ſeriouſly the ſubject of conſideration. In 1778, the late Dr. Hugh Smith, publiſhed his Syllabus of Philoſophical Lectures, in London, four years after Dr. Prieſtley's diſcovery of dephlogiſticated air. We find, in this work, remarks the moſt curious, and an aerial ſyſtem of phyſic beginning to be formed. The nature of Dr. Smith's theory, is ſomewhat analogous to the ideas entertained by Hippocrates, and adopted and enlarged, but confuſed by Liſter. He makes no diſtinction between the air, except between what he calls vital and atmoſpheric, what he thus diſtinguiſhes: Air rarified, in motion, detained in animal bodies by glandular ſecretions, or circulating with the fluids in the vaſcular ſyſtem, permit us to call vital air *. The firſt circumſtance, which led this ingenious phyſician to the theory of air, was his attention to the cauſe of the circulation of the blood. He ſtudied Harvey, whoſe doctrine, that the motion of the blood was

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* The Royal Medical Society of Edinburgh, of which I have the honour of being a member, is one of the uſeful ornaments to this celebrated ſchool of phyſic. Compoſed principally of ſtudents, who have no object in view but the improvement of ſcience, the opinions of profeſſors are canvaſſed with the warmth of youth, and candour of impartiality. Some of the firſt medical characters in the kingdom, have derived their knowledge from this ſociety; and it is with great pleaſure I ſeize the preſent opportunity, of paying my tribute of reſpect and eſteem, to the abilities I have ſeen diſplayed in this learned ſeminary.

† Dr. Prieſtley and Mr. Cavendiſh, in England; Dr. Black, in Scotland; Dr. Macbride, in Ireland; Mr. Scheele, in Sweden; with Meſſrs. Lavoiſier, Fourcroy, and others, in France, were all employed in making thoſe beautiful diſcoveries, which have ſince ſo much added to the improvement of ſcience in general.

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* P. 32.

owing to itſelf, he adopted. But the cauſe of this motion, Harvey did not underſtand, becauſe he knew not the principle; and this cauſe, Dr. Smith pronounces to be vital air.

The hypotheſis of life exiſting in the blood, certainly owes its origin to Moſes, though firſt adopted, philoſophically, by Harvey, who goes ſo far as to ſuppoſe, the blood to be the ſeat of the ſoul. Dr. Harvey's doctrine deprived, altogether, the muſcular fibre, of its ſhare in the functions of the vaſcular ſyſtem. He attributed every thing to the blood; and ſeemed to think, that its motion was owing to a power, inherent in itſelf, without any aſſiſtance from the arterial ſyſtem. Nor is the blood, ſays Harvey, to be called an original and principal part only, becauſe in it and from it, motion, and the beginning of pulſation, ariſe; but alſo, becauſe in it animal heat firſt is bred, the vital ſpirit is produced, and the ſoul itſelf reſides. How infinitely near, adds Dr. Smith, does he approach, and yet does not ſeem to have the moſt diſtant conception, that air was the unknown principle, yet wanting, to produce all thoſe wonderful phænomena, that thus claimed his moſt curious attention? I cannot help conſidering this great man, as my valuable and good ally; but we have a formidable opponent to contend with, one who has endeavoured to overturn the whole of Dr. Harvey's theory, in order to eſtabliſh his own; I mean the celebrated Boerhaave, who abſolutely affirms, that the cauſe impelling the blood, from the heart into the arteries, and from the veins into the heart, is not in the blood, but to be ſought for in the heart itſelf. At this time, the Boerhaavean doctrine of the circulation, was generally received, throughout the medical world; and Smith laments, that his hypotheſis ſhould have obtained the preference over Harvey's. From our obſervations, perhaps, it will appear, that the re-action of the veſſels, which Boerhaave and his followers make the firſt cauſe, is, in reality, only the ſecondary cauſe of the circulation. Both theſe great men, however, obſerves Dr. Smith, though they differed ſo widely in their theories, were neither of them far from the truth. One diſcovered motion in the vaſcular ſyſtem, but knew not the cauſe; the other diſcovered motion in the vaſcular ſyſtem, and miſtook it for the firſt cauſe. It was neceſſary to collect thus much from Dr. Smith's writings, in order to make his doctrine intelligible, which we ſhall now briefly examine.

By a ſeries of experiments, Dr. Smith proved, that vital air, heat, and motion, were inſeparable from animal life. Upon this his theory is built, upon this has he founded all his reaſonings, and from this are all his concluſions drawn. It will be ſufficient to mention his leading facts. Vital air is the material cauſe of motion in animal life; it produces its effects by rarefaction from animal heat. His words are curious, and mark a mind attentive to the ſubject, though ſomewhat confuſed, from the want of a clear and diſtinct idea of terms. In the degree of heat, proper to animal life, this air muſt be in a rarefied and active ſtate. The propelling force of air, thus rarefied and confined in the vaſcular ſyſtem, is the firſt cauſe of the circulation of the blood; and the *re-action* of the vaſcular ſyſtem (his own words) we preſume, to be the ſecondary cauſe, in conjunction with the former. Here we find a part of that doctrine, adopted by Cullen, and afterwards publiſhed, in a more viſionary manner, by Brown The re-action of the veſſels was produced, by the diſtenſion cauſed by the rarefied air; or to ſpeak in more modern language, they acted from the ſtimulus of diſtenſion. This mode of expreſſion is certainly proper; for when the blood is driven from the left ventricle into the aorta, conſiderable effect will be produced, both as to reſiſtance and propulſive force, by the elaſticity of the artery, independent of the contraction produced by the ſtimulus being applied to the irritability of the fibre. It is evident, that Dr. Smith imagined, that air was in a diſengaged and free ſtate in the vaſcular ſyſtem, and that it produced its effects upon mechanical principles. This was deviating, very materially, from Mayow, who, although he did not preciſely underſtand, how fluids paſſed from an aeriform to a condenſed ſtate, yet maintained, that air *united* with the blood, and produced its effects from chemical laws. He drew his concluſions, indeed, from a very different combination of facts, the reſult of cloſely connected experiments. Theſe, although juſtly conceived, and admirably executed, were neglected by many, notwithſtanding the ſame

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idea was entertained and delivered to the Public, under different expreſſions. The vivifying ſpirit of one, with the vital and acid air of others, all meant the fire-air particles of Mayow; and there was only wanting a comparative view of experiments and obſervations, to confirm the beautiful ſyſtem publiſhed by that ingenious author. Recently has this doctrine been revived, in a full and ample manner. The attention of medical men ſeems now principally directed to the agency of air in the human frame; and from the trials that have been made, it would ſeem, that great benefit will reſult to the practice of phyſic.

INTRODUCTION

TO

PART THE SECOND:

COMPRISING THE HISTORY OF THE ABSORBENT SYSTEM.

THE nature and functions of the abſorbent ſyſtem, is a ſubject which ſtill conſiderably engages the attention of the phyſiologiſt; and it is ſuppoſed, that, but lately, this part of our frame has been rightly underſtood. Immediately upon the diſcovery of the lacteals, the office aſſigned to them was the reſult of the idea entertained, of the nature of the circulation. But as ſoon as that great light was let in upon phyſiology, by the immortal Harvey, and when the diſcoveries of Rudbeck, of Bartholin, and of Jolyffe, were known, the firſt abilities were exerted, in explaining this important part of the animated fabric, and the futility of abſorption by red veins, and of the lacteal carrying chyle to the liver, was ſeen and acknowledged.

The diſcovery of the lacteals, by Aſellius, forms an æra in the hiſtory of medicine. But, notwithſtanding that he undoubtedly has a ſole right to this diſcovery, yet we find, that others, about the ſame period, maintained, that they ſaw the lacteals, without knowing of Aſellius's diſcovery. Highmore puts in this claim, but, at the ſame time, gives up the right of diſcovery to Aſellius, as Bartholin ought to have done that of the lymphatics to Rudbeck.* When diſſecting a dog, ſays Highmore, I accidentally ſaw theſe veſſels, without having read of them in any one author; at firſt, I could only trace them to the pancreas, but afterwards, I acquired a more perfect knowledge of them, from Aſellius.

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* Circa eadem tempora, in Suecia, ſimiles aquoſos ductus delexit et poſtea deſcripſit Olaus Rudbeck. In Anglia de Jolivio quoque ſuo gloriantur amici. Quin nobiſcum aliis hinc inde viſa ſunt, negare nolim ſed nobis qui primi in arenam deſcendimus, nullus mortalium viam monſtravit. Anat. Renov. p. 622. Upon the ſame grounds, Highmore has a right to the diſcovery of the lacteals.

Aſellius diſcovered theſe veſſels, in brutes, in 1622; and Highmore ſays, that an Oxford phyſician mentioned to him, that he ſaw them in the human ſubject, in 1637, and that he himſelf ſaw them, alſo, in the human ſubject, in 1639, and ſhewed them, diſtended with chyle, to a number of pupils; but Veſlingius, according to Haller, firſt ſaw the lacteals in men, in 1634. Mr. Cruikſhank, in his ingenious work, points out, that abſorption was maintained, but not underſtood, both by Hippocrates and Galen; and that Herophilus, according to the latter, had ſeen the lacteals. Highmore mentions the ſame thing, and quotes a remarkable paſſage from Fallopius, who certainly ſaw the lymphatics, from the accurate deſcription he has given. On the flat ſide of the liver, are certain very ſmall ducts, which terminate in the pancreas,* and neighbouring glands, and which convey a white oleaginous juice. The lacteals then, were firſt ſeen by Herophilus; but their nature and functions were not underſtood by anatomiſts, until Aſellius again held them up to the notice of the medical world, when Veſlingius, Highmore, and others, notwithſtanding that the exiſtence of ſuch veſſels was denied by the great Harvey, by ſeeing them in men, confirmed the diſcovery. The lymphatics appear to have been firſt ſeen by Fallopius; it is evident, that Aſellius alſo ſaw them, but miſtook them for lacteals. This ſubject remained confuſed, until Rudbeck and Jolyffe again diſcovered them, when Willis, Gliſſon, Bartholin, and others, completed the ſubject. The thoracic duct, according to Mr. Cruikſhank, was firſt ſeen and deſcribed, in 1563, by Euſtachius, a Roman anatomiſt; but this diſcovery was alſo neglected by anatomiſts, till Pecquet and Van Horne tacitly cenſured this neglect, by confirming the truth of it. Such, conciſely, are the revolutions, which the knowledge of the abſorbents ſuffered, for many years, till the middle of the laſt century, when the ſubject underwent a full inveſtigation, as the reader will ſee in the ſubſequent part of this work.

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* Pancreas aſelli, the meſentery.

This ſubject, however, ſeems not to have employed much conſideration, for ſeveral years, previous to the publications of Hunter and Monro.

It is well known, that theſe celebrated anatomiſts declared themſelves, the diſcoverers of the identity of lacteals and lymphatics; that is, that both theſe ſects of veſſels abſorbed from cavities or ſurfaces, and at times carried the ſame kind of fluid, and therefore, in that point of view, were ſimilar, in reſpect of office; and that, alſo, they were to be conſidered as the ſame ſyſtem of veſſels, from a ſimilarity of ſtructure*. Now it will be no difficult matter to prove, that theſe ideas were entertained and adopted, long before Dr. Monro or Dr. Hunter ever thought upon the ſubject. Mr. Cruikſhank, in his valuable treatiſe on this ſubject, maintains, that Dr. Hunter ſtill has a claim to the diſcovery. Having obſerved, that Dr. Hunter maintained his claim ſo well, that the greater part of thoſe who were entitled to hold any opinion, were on his ſide, he adds; when Harvey diſcovered the circulation of the blood, his opponents firſt attempted to prove he was miſtaken; but finding that ground untenable, they then aſſerted it was known, long before: Servetus, Columbus, and Ceſalpinus, all knew it*. It is very remakable, that thoſe obſervations ſhould be brought to invalidate arguments, brought againſt Dr. Hunter's pretenſions, when we find that gentleman himſelf ſtrongly aſſerting, that the diſcovery of the circulation was made prior to Harvey. Speaking of abſorption, he adds, Gliſſon having been quoted, I conſidered what he had advanced upon this ſubject, and had the pleaſure and mortification to find, that he gave exactly the ſame account, both of tranſudation and abſorption; ſo that I could no longer call it, what I really believed it to be, a new opinion, but Gliſſon's revived and confirmed: for in him it was mere opinion, and accordingly was overlooked or rejected, by his ſucceſſors, as happened to the doctrine of the circulation, in the writings of Servetus and Ceſalpinus*. But further, in his introductory lectures to anatomy, we find the following obſervations: Servetus firſt, and Columbus afterwards, had clearly given the circulation of the blood through the lungs, which we may reckon, at leaſt, three-quarters of the diſcovery; and Ceſalpinus had, many years before Harvey, publiſhed, in three different works, all that was wanting to make the circulation quite complete†. Having mentioned the great merit which is due to Columbus, for the diſcovery of the Weſtern Hemiſphere, and to Copernicus, for that of the Solar Syſtem, he adds; In merit, Harvey's rank muſt be comparatively low indeed. So much had been diſcovered by others, that little more was left for him to do, than to dreſs it up into a ſyſtem; and that, every judge in ſuch matters muſt allow, required no extraordinary talents. Yet eaſy as it was, it made him immortal: but none of his writings ſhew him to have been a man of uncommon abilities‡. It is unneceſſary for me to me to uſe any argument, to prove the comparative merits of Harvey: it is foreign to my purpoſe. As Mr. Cruikſhank aſſerts Dr. Hunter's right to the diſcovery of the lymphatics being abſorbents, I ſhall chiefly confine my obſervations to his work, particularly too, as containing the opinions of Dr. Hunter; and all the arguments and obſervations will be equally applicable, to whatever ſhare of diſcovery Dr. Monro may have claimed, in this important branch of phyſiology. It is, by no means, our intention, to enter into the diſpute between Dr. Hunter and Dr. Monro: our object is to ſhew, that the doctrine of tranſudation and abſorption, was a poſition maintained by the firſt diſcoverers, and conſiderably improved by the anatomiſt who immediately ſucceeded them.

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* Vide Lect. on Anat. p. 58; and De Vaſis Lymph. Valv. p. 103; and Prolegom. p. 8.

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* Introduction, p. 3. We have been unable to procure the works of Servetus and Ceſalpinus. The anatomy of Columbus is in our poſſeſſion; and he certainly makes ſome ſtriking obſervations, on the circulation of the blood, through the lungs: but notwithſtanding this, he maintained, that ſanguification was performed by the liver, according to the Galenic doctrine. At ſome future day, if time will permit, it is our intention, to examine minutely the doctrines that were entertained, reſpecting the circulation, previous to Harvey.

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* Med. Comment. p. 61.

† P. 44. ‡ P. 47.

We flatter ourſelves, no apology is neceſſary to the gentlemen, whoſe doctrines we have ſo freely canvaſſed. The moſt oſtenſible modern author, on the ſubject of the abſorbents, is Mr. Cruikſhank, whoſe liberality will not permit him to ſuppoſe, we had any thing elſe in view, than a candid and impartial enquiry, by the freedom of our ſtrictures on his opinions.

PART THE SECOND.

SECT. I.

Of ABSORPTION *by* RED VEINS.

BEFORE we proceed to examine, what different opinions eminent anatomiſts maintained, as to the office of lacteals and lymphatics, let us firſt ſee, what ideas they had, as to abſorption by red veins. Bartholin ſtrenuouſly maintains, that no abſorption takes place by the meſeraic veins. His words are. (A) Their uſe, according to the antients, is to ſuck up chyle from the inteſtines, and to carry it, by the trunk of the vena portæ, to the liver; but the chylous fluid is never found in theſe veſſels, they being always filled with blood: moreover, the diſcovery of the lacteals and lymphatics, diſprove ſuch an abſorption. That chyle is abſorbed by the lacteals alone, is proved, by an experiment of Barbett. He paſſed ligatures under ſome part of the inteſtines of a living dog: between the ligatures, the meſenteric veſſels, both red and white, came into view; ſome of theſe he tied with a thread, and afterwards inflated the inteſtine with force, through a pipe; but he found, that not the red veins, but only the lacteals became turgid *. And a little lower down, he adds; (B) ‘ Hence the meſenteric veins can abſorb nothing from the inteſtines. Riolan, however, aſſerts, that in obſtructions of the lacteals, the chyle muſt, from neceſſity, be carried through theſe veſſels; but this is by no means the caſe. Nor do theſe veins open into the inteſtines; for then the blood would be effuſed, and, in my opinion, nouriſhment would rather ceaſe, as happens in lientery, from obſtruction of the lacteals.’ When ſpeaking of the lacteals, he obſerves, (C) ‘ Their proper uſe and action is to diſtribute chyle to the liver, according to Aſellius; but rather to the receptacle, and through the thoracic duct to the heart, as Pecquet and ourſelves have obſerved; not by the meſeraics, as heretofore believed, through which there is neither a diſtribution of chyle to the liver, nor of blood to the inteſtines.’ He combats, by powerful arguments,

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* This experiment did not ſucceed with Lower;—vide infra, Sect. III.

guments, the experiments which De Bils had made, to prove abſorption by the meſenteric veins; and obſerves, that from their ſtructure, it is impoſſible for them to abſorb. (D) ' The meſeraic veins are ſo conſtructed, that they cannot take up chyle by their mouths.' In Walœus we find the following obſervations: (E) ' Chyle is never obſerved to enter any vein in the ſtomach, nor any meſenteric vein. The chyle is carried through veſſels, ſuited to the purpoſe rather than through the meſeraic, which contain blood; for then it would be neceſſary, that theſe veins ſhould open into the inteſtines, by which the blood would eaſily eſcape.' We find, alſo, another celebrated anatomiſt, making direct experiments, to diſprove the abſorption by red veins. (F) ' And this is the only way, ſays Lower, through which the chyle paſſes, from the ſtomach and inteſtines, to the blood and heart. Becauſe ſome have fallen into the ſame error with the antients, and ſtrenuouſly maintain, that the meſeraic veins take up chyle from the inteſtines, I have paid particular attention to this ſubject, that I might be certain of the fact; and I at length found, not from one experiment only, that the whole ſupply of chyle is thrown into the blood, by no other paſſage but the chyliferous ducts; for if its paſſage through theſe veſſels be obſtructed, the animal, with whatever kind of food it may be fed, will die in a few days from famine, as I have experienced in two dogs. He lacerated the thoracic duct, and obſerved, that notwithſtanding the ſtomach and inteſtines were diſtended with food, and the lacteals with chyle, yet the dogs died. He was aware, he ſays, that the thoracic duct was ſometimes double; he, therefore, made the inciſion high enough up, in order to be above the junction: he found the chyle effuſed in conſiderable quantity into the thorax from the lacerated duct; (G) An evident and clear proof, he adds, that the chyle does not enter by the meſeraic veins, and that it has no other way by which it is carried to the blood, ſince the animal is ſo certainly deſtroyed, if the paſſage through the thoracic veſſels be cut off.' Surely no candid mind can refuſe its aſſent to the convincing arguments drawn from theſe experiments. It can be no violence to reaſon to obſerve, that both Bartholin and Lower ſaw the *neceſſity* for different functions, in the abſorbents and red veins. They beheld and admired the effectual ſimplicity of nature, in the animal œconomy; hence they reaſoned on the futility of red-vein abſorption; and hence they maintained, with a firmneſs which always attends the aſſertion of truth, and proved by direct experiments, their opinions to the contrary.

In Diemerbroeck, too, we find a ſtrenuous aſſertor of the non-abſorption by red veins. When ſpeaking of the different uſes that have been aſſigned to the meſenteric veins, and the objections ſtarted againſt them, he adds; (H) 'Plempius, perceiving theſe difficulties, explains the ſubject otherwiſe, and ſays, that the arterial blood remaining after the nouriſhment of the inteſtines, flows back by the meſeraic veins to the porta, and along with it the chyle. But I wiſh Plempius would demonſtrate to us, the way by which the chyle enters theſe veins. It is neceſſary for the reception of the chyle, that they ſhould, in ſome manner, open into the inteſtines; if this is the caſe, why does not the blood flow through theſe apertures into the inteſtines? and again, in the following paſſage, his expreſſions are remarkably ſtrong.

(I) ‘ Beſides, no one could ever ſee chyle, nay not any thing which bears the ſmalleſt reſemblance to chyle, in the meſeraic veins, but the white chyle is always found in the lacteal and other chyliferous veſſels, which were conſtructed by the Supreme Architect *to this expreſs purpoſe, viz. that they ſhould convey the chyle.*’ He mentions the experiments of De Bils, who ſeparated from each other, in a living dog, the meſenteric veins and arteries, and paſt ligatures round the latter, which prevented any blood from paſſing from the arteries into the veins. He then ſewed up the abdomen, and preſerved the dog, until he imagined the food, which he had previouſly given the animal, was converted into chyle. He then found the arteries empty, but diſcovered the veins filled with a muddy fluid, of a cineritious colour. This experiment of De Bils greatly excited the attention of the phyſiologiſts. Some of the firſt medical characters of the age entered the liſts, and the diſpute was carried on with that acrimony which has, too often, diſgraced literary controverſies. (K) All the Bilſianiſts, ſays Diemerbroeck, regard this experiment as miraculous, and aſcribe ſo much to it, that they are aſtoniſhed if any one ſhould dare to contradict it; moreover, they revile, with the moſt baſe and ſharp contumelies, language unworthy literary characters, thoſe who oppoſe the opinion.

The names of Steno, Bartholin, and Diemerbroeck, are found among the diſputants; and the latter, after having brought forward the experiments and opinions of others againſt red-vein abſorption, cries out exultingly: (L) 'We ourſelves have repeatedly collected blood from the meſeraic veins, and at that time, too, when all the lacteals were replete with chyle, and have compared it with the blood of other veins; but we were never able to diſcover the ſmalleſt difference, in colour, thickneſs, or coagulation. The ſame, alſo, was taken notice of by that moſt accurate obſerver, Nicolaus Steno'. Van Horne, too, one of the diſcoverers of the thoracic duct, did not believe in abſorption by red veins; and Bartholin, in his Structura nova Diaphragmatis, mentions ſome authors who maintained ſuch an opinion, and refutes their arguments*.

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* Notwithſtanding theſe unequivocal experiments and

obſervations,

(A) Uſus, ſecundum antiquos, chylum exſugere ex inteſtinis, et per truncum venæ portæ in hepar deferre; ſed nunquam in his lacteus chyli ſuccus repertus, ſemper ſanguine refertis: adhæc inventio venarum lactearum et lymphaticarum iſti repugnat. Per ſolas lacteas chylum ſugi hoc experimento probat Barbettius. Prehenſum quâvis parte canis vivi inteſtinum utrinque ligat: in medio hujus exprompta meſenterica vaſa, tum alba tum rubra, aliquot itidem filo nectit: poſt per adaptatam fiſtulam fortiter flatum injicit; ſic non ligata ſanguinea, ſed ſola lactea intumeſcere vaſa comperit*, P. 602-3.

(B) Hinc

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obſervations, by different eminent anatomiſts, Dr. Hunter, in his Med. Comment. after relating the experiments of John Hunter, to diſprove abſorption by red veins, ſays, here is a *new doctrine* propoſed in phyſiology, *viz.* that red veins do not abſorb.

* Thomæ Bartholini Anatome ex omnium veterum recentiorumque Obſervationibus, &c. Renovata. Lugduni Bat. 1673.

(B) Hinc nihil ex inteſtinis meſaraicæ ſugere poſſunt. Neceſſitatis tamen tempore in omnimoda obſtructione venarum lactearum, per hæc chylum deferre concedit Riolanus, ſed nullo argumento. Nec enim hiant in inteſtina, alioquin effunderetur ſanguis, et meo judicio, ceſſaret potius nutritio, quod in lienteria videre eſt, illis obſtructis. Ibid. P. 603.

(C) Actio et uſus propius eſt chylum per *ἀνάδοςιν* ad hepar diſtribuere juxta Aſellium; ſed ad receptaculum verius, indeque per thoracicas lacteas ad cor, ſecundum Picqueti noſtraſque obſervationes; *non per meſaraicas*, ut hactenus creditum, per quas nec fit chyli *ἀνάδοςις* ad hepar, nec *διαδοςις* ſanguinis ad inteſtina. Ibid. p. 613.

(D) Venæ meſeraicæ ita ſunt comparatæ, ut chylum per oſcula ſua non recipiant. Ibid. P. 606.

(E) Nunquam autem chylus animadverſus eſt venam ullam in ventriculi corpore, nec ullam meſaraicam intrare. P. 765*. Per peculiares autem

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* Epiſtolæ Duæ de Motu Chyli et Sanguinis; annexed to the Anatome Renovata.

venas, potius quam per meſaraicas ſanguinem continentes, defertur chylus; quod meſaraicæ ſanguinem admiſſuræ, aperiri oſculis in inteſtina debuiſſent, per quæ facile ſanguis in inteſtina fuiſſet elapſus. Ibid. P. 789.

(F) Atque hæc via unica eſt, qua chylus e ventriculo et inteſtinis, in ipſum ſanguinem et cor infunditur. Verum quia nonnulli in eodem cum veteribus errore etiamnum verſantur, venaſque meſaraicas chylum ex inteſtinis excipere confidenter ſtatuant, ipſe ut de hac re certior fierem, ſeriam aliquando impendi operam; atque non uno experimento tandem mihi conſtitit, totum chyli penum *nulla alia via* quam per ductus chyliferos, in ſanguinem infundi; ſi enim curſus ejus per vaſa thoracica impediatur, arimal qualicumque cibo ſatiatum, intra paucos dies fame penitus interibit, quod in duobus canibus expertus ſum. P. 153-4 *.

(G) Claro ſatis argumento, chylum per venas meſaraicas non intrare, neque ullam aliam dari viam qua ſanguini miſceatur; cum animal, ex tranſitu per vaſa thoracica impedito, tam certò intereat. Ibid. P. 155.

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* Tractatus de Corde item de Motu et Colore Sanguinis et Chyli in eum tranſitu; Authore R. Lower, M. D. Coll. Med. Lond. Socio. Londini, 1680.

(H) Plempius, has difficultatés percipiens, rem aliter explicat, et dicit, ſanguinem arterioſum, a nutritione inteſtinorum reſiduum, per venas meſaraicas refluere ad portam, eique chylum ex inteſtinis adjungi vel permiſceri. Sed velim nobis demonſtret Plempius, vias per quas chylus venas illas ingreditur. Illæ enim ut chylum recipiant neceſſario debent alicubi in inteſtinis aperiri; at quare, dum aperiuntur, ſanguis, qui tenuior et ſpirituoſior eſt chylo, per iſtas aperturas non effluit potius in inteſtina? P. 724-5 *.

(I) Præterea, nemo unquam chyli, imò ne minimam quidem chyli ſimilitudinem, in venis meſaraicis obſervare aut videre potuit; ſed chylus albicans ſemper invenitur in vaſis lacteis, aliiſque chyliferis, *eum in finem*, ut *chylum veherent*, a Summo Architecto ſpeciatim conditis. Ibid. P. 725.

(K) Hoc experimentum omnes Bilſianiſtæ, tanquam miraculoſum admirantur, idque tanti faciunt,

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* Iſbrandi de Diemerbroeck Anatome Corporis Humani. Genevæ, 1679.

unt, ut obſtupeſcant, ſi quiſpiam aliquid contradicere audeat; imo turpiſſimis acerrimiſque contumeliis ac convitiis, viris doctis indigniſſimis, quoſcunque contradictores proſcindant. Ibid. P. 725.

(L) Nos enim non ſemel ſanguinem e meſaraicis collegimus, etiam eo tempore, quo omnes lactei ductus chyloſo ſucco turgebant, eumque cum aliarum venarum ſanguine contulimus; ſed nullum manifeſtum diſcrimen in colore, ſubſtantia, aut coagulatione, animadvertere potuimus. Illud ipſum etiam obſervavit oculatiſſimus Nicolaus Steno. Ibid. P. 726.

SECT. II.

ORIGIN *of* LACTEALS *and* LYMPHATICS.

It appears, from reading the works of thoſe who have written upon the abſorbents, that, notwithſtanding ſome of them maintained, that the lymphatics were continued from arteries, many, alſo, ſupported the opinion, that they aroſe from ſurfaces and cavities. Mr. Cruikſhank, in ſeveral parts of his work, maintains, that Bartholin knew nothing of the origin of the lymphatics. I agree with him in thinking, that he ſometimes had his doubts; but ſhall we ſay, becauſe he heſitated, that he therefore was totally unacquainted with it? But he ſeems to be tolerably clear, on this ſubject, in ſome places. (A) ' The origin of the lymphatics is from all parts, and the viſcera, from which lymph is *ſeparated*, the liver, the gall-bladder, the ſternum, mediaſtinum, pericardium, heart, lungs, inteſtines, ſtomach, ſpleen, uterus, teſticles, &c. The uſe of all the lymphatic veſſels is, to carry a limpid fluid or lymph; the lymph is produced from all parts, throwing out a ſuperfluous ſerum, or from the arteries abounding with lymph.' Can any poſition be more clearly ſtated? He firſt lays it down, as certain, that the uſe of the lymphatics is, to carry lymph; and he then ſays, that the lymph is thrown out, upon different parts. What concluſion are we now to draw? It is ſo evident, that it would be doing violence to common ſenſe, to mention it. He aſcribed to the lacteals and lymphatics, the abſorption of medicines, poiſons, &c. hence, he ſays, we may account for many diſeaſes, as dropſy, &c. and we can now ſee, he adds, the cauſe of the conſent between the ſtomach and heart. He obſerves, that theſe veſſels ſerve to complete the human frame; and concludes, that as to the diſeaſes he has mentioned, he will treat of them fully, in a book, De Morbis Lymphaticis, a work we have never ſeen, but which, it is very probable, contains many obſervations and opinions, ſuppoſed to be modern. In a work publiſhed by Bartholin, after the Anatome Renovata, which came into our hands after the above was written, he gives a moſt decided opinion, that the lymphatics are not continued from arteries, by continuity of canal: (B) ‘ Moreover, although we aſſert, that the lymphatic veſſels receive their contents from the arteries, *we by no means allow of a direct anaſtomoſis of the lymphatics with the arteries*, except through the medium of cells, or ſome other ſtructure, into which the blood being effuſed (a curious idea) and having ſupplied to the nutrition of parts and ſecretion of fluids, the lymphatics return the lymph, the veins the blood.’ Other celebrated anatomiſts paid particular attention to the ſame doctrine, and by their expreſſions, we cannot doubt, but that they had a clear and adequate notion of the ſubject. Paulus, the editor of Van Horne's Opuſcula, has the following words, when ſpeaking of the liver; (C) ‘ Indeed thoſe lymphatics, as well as the others of the whole body, do not ariſe ſo much from membranous parts, as from the interſtices where the arteries terminate.

It was the opinion of Steno, Ruyſch, and Malpighi, that the lymphatics aroſe from glands; and this opinion is adopted by Diemerbroeck. Bartholin denies, that they ariſe ſolely from glands. (D) ‘ My honoured father, he ſays, who firſt gave name and eternity to the veſſels diſcovered by him, believed, that the lymphatics returned the lymph into the blood, from all parts, as well glandular as the extreme joints.' He mentions, that they had been ſeen coming from parts not glandular; and that many laughed at De Graaf, for maintaining, that he had ſeen them upon the uterus. This author, in ſpeaking of the lymphatics of the teſticles, has the following words. (E) 'The lymphatic veſſels, which the curioſity of our age has ſo admirably diſcovered in the teſticles, are ſeen not only to come out from the tunica albuginea, but alſo from the internal ſubſtance of the teſticles.' Theſe lymphatics, as well as the reſt, he adds, have numerous valves to prevent the return of the lymph to the teſticles, and alſo to cauſe it to be propelled, with more eaſe, to the receptaculum chyli. Reverhorſt gives a very particular and accurate account of the lymphatics of the liver, of which, he ſays, there are two ſets; the deep ſeated and the ſuperficial. He mentions, that before they quit the liver, they form themſelves into ſix, ſeven, or eight large trunks, and ſometimes more, paſſing along both ſides of the ſuſpenſory ligament. A circumſtance which he mentions, clearly proves, that the lymphatics could not be continued from the arteries, by continuity of canal. (F) Theſe lymphatics, he ſays, both deep ſeated and ſuperficial, tranſmit lymph, very different from that of the other lymphatics, becauſe it is impregnated with bile, and therefore may be deſervedly called bilious lymph. This is alſo taken notice of by Kirchringius, as quoted by Bartholin, who mentions it to prove, that the lymphatics are not continued from the arteries. (G) This is alſo proved for us by Kirchringius, who, becauſe he ſaw the lymph tinged of a yellow colour, in a lymphatic creeping along the top of the gall-bladder, and perceived a taſte of bile, concluded, that this bitter lymph was taken up, *without the veſſels of the arteries*; becauſe if there were an anaſtomoſis between the arteries and lymphatics, the lymph would not be found bitter, but ſweet, as it is detected in the arteries*.

The ingenious Thruſton, when ſpeaking of the nouriſhment of the fœtus in utero, in the examination for his degree at Cambridge, ſays, that the fluids, in which it is immerſed, paſs through the ſkin. Dr. Needham, the profeſſor, queſtioned him, as to this particular, to which he gave the following anſwer; (H) ' Nor do I think that you will deny, that ſomething paſſes through the ſkin, in the capillary veſſels, and thus mixes with the blood. I believe, that you (ſo long and ſo ſucceſsfully employed in the practice of phyſic) not unfrequently order fermentations; ſuppoſe you are preſcribing for ſchirroſities of the abdomen, can you doubt, but that ſome part of them enter through the ſkin, into the ſanguiferous veſſels? Here we find abſorption maintained to be performed by the cutaneous veſſels. The diſcovery of the lymphatics was made ſeveral years previous to this period; and the doctrine, that fluids were removed by them, had been for ſome time mentioned and acknowledged: it is probable, therefore, that Thruſton might imagine, this office to be performed by the cutaneous abſorbents; although I do not deny, that his words will bear a different interpretation. If a candid reader, therefore, puts a different conſtruction upon the paſſage, we willingly reſign our idea, as our object is, not to ſupport, unjuſtly, the pretenſions of any one man, but to promote, by diſpaſſionate enquiry, the knowledge of truth.

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* De Graaf gives a very neat plate of the uterus, with a deſcription of the lymphatics running along it.—De Mulier. Organ. P. 247.

In a work publiſhed a few years after the Anatome Renovata of Bartholin, we find an excellent hiſtory of the abſorbent ſyſtem.* Zellerus, the author of this publication, gives the following unequivocal account of the origin of the lymphatics. (1) 'Some believe, that theſe veſſels come out from nouriſhed parts; others hail all the greater conglobate glands, as the fountain of all of them; others, again, deny this, ſince they are found in parts, as the lungs, where no glandular ſtructure obtains. Almoſt *all of them are in the right.* We pronounce the glandular membrane, which is extended over all the muſcles, in a word, over all parts, and the viſcera as *their commencement*, *fountain*, and *origin*, *ſince we may ſee them ariſing from all parts*, as well from the conglo-

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* Bartholin's work came out in 1674; Zellerus's account of the lymphatics is publiſhed with the Diſputationes Anat. Select. by Waller. Mr. Cruikſhank ſays, he never ſaw Zellerus's publication.

conglomerate as conglobate glands, from the liver, pancreas, teſticles, the viſcera, from the joints, the abdominal lumbar, and intercoſtal muſcles, from the diaphragm, pleura, pericardium, and membranes of the brain.'* (I) Long, too, before the time of Zellerus, Van Horne maintained, that abſorbents were diſtributed over the whole ſyſtem. (K) The diligent inveſtigation of our more recent anatomiſts has taught us, that there is ſcarcely any part of our body, which wants theſe veſſels; for they are found in the lungs, mediaſtinum, ſtomach, ſpleen, teſtes, uterus, ſeptum narium, eye-lids, palate, cheeks, &c.

The ſeveral paſſages here ſelected, point out, beyond a doubt, that ſome of the firſt diſcoverers of the abſorbents, knew their general extent and influence over the whole ſyſtem. They ſhew clearly, too, that the phyſiologiſts of the laſt century knew, that the lymphatics aroſe from cavities and ſurfaces, and that they were not returned from the arteries, by continuity of canal; yet Dr. Hunter maintains, that this was the opinion entertained by anatomiſts ſince the diſcovery of the abſorbent ſyſtem, notwithſtanding we find Bartholin himſelf, ſo anxious and careful in aſſerting the contrary doctrine.

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* Dr. Morelli, Profeſſor of Medicine at Padua, and who ſtudied for five years under Muſcagni, mentioned to a friend of mine, that he has frequently ſeen lymphatics, running between the arachnoid coat and pia mater of the brain.—Vide Nuck's Epiſt. Anatom. P. 150; where he mentions, that he has ſeen lymphatics paſſing from the pineal gland.

SECT. III.

ORIGIN *of the* LACTEALS.

There could be but one opinion, as to the part whence the lacteals derived their origin; but the *manner* in which they proceeded, from their commencement, formed a ſubject for diſpute, which, by the way, points out, how much the authors of laſt century were occupied with this part of phyſiology. Lower imagined, that the lacteals aroſe obliquely from the inteſtines. Having firſt fed a dog, he collected air in that portion of the jejunum, where ileum commences, and having applied ligatures above and below, he found no air in the lacteals with the chyle, after repeated preſſure with the hand, before he untied the ligatures. He repeated this experiment, he ſays, but made uſe of a coloured fluid inſtead of air; ſpiritus vini atramento tinctus; the reſult, however, was the ſame: hence, he concludes, (L) 'from

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this we have reaſon to think, that the lacteals do not open directly and immediately into the inteſtines, but run obliquely among their coats, before they penetrate the inteſtinal cavity; in the ſame manner, perhaps, as the ductus communis choledochus enters the duodenum, or the ureters the urinary bladder.*

This is the opinion of Lower. Whether it has been confirmed or refuted by modern experiments, I cannot pretend to ſay; but it appears, that ſuch an oblique commencement is contrary to the deſign of Nature, by throwing obſtacles in the way of a ready abſorption. The numerous mouths which the lacteals preſent in the villi of the inteſtines, ſhews, how careful Nature is, to promote the grand and neceſſary office of theſe veſſels, *viz.* the abſorption of chyle: and the beautiful deſcription, which the accurate Cruikſhank has given, of the numerous mouths which he ſaw, deſerves the attentive peruſal of the phyſiologiſt.†

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* Vide the Experiments of John Hunter, as related in his Brother's Med. Comment. P. 38. The reader will, probably, be ſurpriſed at experiments ſo ſimilar.

† Vide Chap. xi. of the Anatomy of the Abſorbing Veſſels.

Other writers, previous to this ingenious anatomiſt, have given deſcriptions of the origin of the lacteals, as if they ſaw their mouths; but as they do not poſitively aſſert it, we cannot declare for them. (M) ‘ They (the lacteals) are inſerted, ſays Bartholin, into all the inteſtines, into which they open with their mouths, ſurrounded with a cruſt of mucous matter; and leaſt the chyle, once received, ſhould return into the inteſtines, they are beſet with valves.’ Diemerbroeck, too, in his anatomy, gives a ſimilar, but more accurate deſcription, of the manner in which the lacteals take their origin. (N) They ariſe, ſays he, from the inteſtines, between the coats of which they open into their interior cavity, by numerous very ſmall terminations, their mouths being concealed under a kind of ſpongy mucus.’ This ſpongy mucus, was probably the ampullula of Liberhuhn, and the villi ſo often mentioned by anatomiſts.

It is uſeleſs to give quotations from more authors, on this part of our ſubject; they have arranged their ideas in almoſt ſimilar terms, which convey to us, that the doctrine generally adopted, was that which we have juſt now mentioned. They form deſcriptions, however, very analogous to thoſe we find given in modern books, on this ſubject, which oblige us to agree, if they did not actually ſee the mouths of the lacteals, that their ideas are very accurate.*

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* Vide Hewſon, Cruikſhank, Monro, &c.

SECT. IV.

STRUCTURE *of* LACTEALS *and* LYMPHATICS.

The abſorbents are, every where, deſcribed by the firſt diſcoverers, as tranſparent, pellucid veſſels, carrying a limpid fluid; hence they have been called by the different appellations of vaſa cryſtallina, rorifera, ſeroſa, &c. This pellucid appearance aroſe, it was imagined, from their delicate ſtructure; and from this circumſtance, they are mentioned as poſſeſſing but one coat. (O) 'The lacteals, ſays Diemerbroeck, are ſlender, pellucid veſſels, having but one coat; and being diſperſed, in infinite numbers, over the meſentery, are deſtined to carry the chyle.' This error, however, was ſoon corrected, by the ingenious Nuck, who ſeparated the thoracic duct into two coats. He gives the following deſcription of a lymphatic. (P) 'The membrane compoſing a lymphatic veſſel, when examined by the naked eye, appears pellucid and ſlender, and the external ſenſes can diſcover no peculiar ſtructure, on account of the minuteneſs of the parts. But examined by the microſcope, it preſents a texture, which exhibits an infinite number of globules, great and ſmall, in contact with each other; ſome of which ſeemed to form ſmall, but irregular ducts, united by various anaſtomoſes, like the meſhes of a net; but whether theſe lines were veſſels or not, I cannot for certain determine.'*

The moſt important part, in the ſtructure of the abſorbents, are the valves, which are more numerous in theſe veſſels, than in any other of the human body. Theſe, too, notwithſtanding their minuteneſs, did not eſcape the inveſtigation of the anatomiſts, who were acquainted with the abſorbents. (Q) ' They are knotty, ſays Bartholin, and appear externally unequally ſwelled, eſpecially if the flow of the lymph be ſtopt by ligature; this happens, on account of the valves, which are not only affixed at the entrance into the axillary vein, but are alſo *diſpoſed*, *every where*

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* Were theſe not the vaſa vaſorum, which Nuck ſaw? confounding them probably with the fibres of the lymphatic.

where double, and in pairs, through all their branches; are very ſlender in their ſtructure, and more numerous than in the veins, ſo much ſo, that ſome anatomiſts have demonſtrated above two thouſand in the lymphatics; their uſe is, to prevent the retrograde motion of the lymph.' So minute a deſcription, could have been the reſult only of laborious attention; and ſo indefatigable was their induſtry, that they attempted even to enumerate them. It is, notwithſtanding, aſſerted by Mr. Cruikſhank, that the diſcoverers of the lymphatics were not acquainted with their valves. 'One is ſurpriſed to find, he ſays, that one of the firſt diſcoverers of lymphatics was almoſt *entirely ignorant* of the valves; and that, after the veſſels themſelves had been deſcribed, both by Bartholin and Rudbeck, the dilucidatio valvularum ſhould have been left for Ruyſch.'* It is very certain, that Ruyſch was the firſt who publiſhed, ſeparately, upon the valves; but ſurely he was not, on that account, the firſt who knew any thing of them: on the contrary, it appears, that Bartholin was as well acquainted with their ſtructure and uſe, as Ruyſch himſelf. This laſt anatomiſt, too, cenſured Rudbeck, for being ignorant of the valves; but, according to Mr. Cruikſhank himſelf, he is very diſtinct and clear upon the ſubject. Gliſſon, alſo, who wrote long before Ruſych, but after Rudbeck, and to whom Jolyffe firſt mentioned his diſcovery of the lymphatics, thus ſpeaks of the courſe of the lymph: (R) ‘ It clearly appears, that this fluid flows from the liver, and by no means towards it; this is alſo evidently proved, by the ſtructure of the valves, which are very numerous in theſe veſſels; they yield a ready paſſage to the fluid, coming from the liver, but altogether prevent its return.’

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* P. 63. Mr. Cruikſhank here quotes a paſſage, from ſome work of Bartholin, in which he ſays, 'valvulæ, ob ſubtilem contextum, cultro anatomico ſeparari non poſſunt. This is almoſt directly contradictory to the one we have ſelected, and in which he deſcribes the valves ſo well. We could

Any one who ſees a turgid abſorbent, muſt obſerve conſiderable inequalities in its ſurface.† So

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could not find the paſſage advanced by Mr. Cruikſhank, as he has given no references to the authors he has quoted.

† It is not neceſſary to open the veſſels, in order to

So ſtriking and unuſual a ſtructure, muſt have arreſted the attention, and have induced the enquirer to examine into this curious appearance; and a ſimple diſſection would ſoon explain the cauſe. Accordingly, we find the valves mentioned, and accurately deſcribed, by the firſt diſcoverers, and the braided, or *nodulated* appearance, is delineated, in the engravings of Bartholin, Willis, Gliſſon, Reverhorſt, and innumerable others.

(A) Exortus lymphaticorum vaſorum eſt ab omnibus partibus et viſceribus, ex quibus lympha ſeparatur, hepate nempe veſicula fellis, ſterno, mediaſtino, pericardio, corde, pulmonibus, inteſtinis, ventriculo, liene, utero teſtibus, &c. Uſus vaſorum lymphaticorum omnium eſt, liquorem limpidum ſeu lympham vehere; origo lymphæ

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diſcover the valves; they are ſufficiently marked on the outſide, and give the veſſels the appearance of being notched or jointed, in different places. The lacteals are, generally, ſtrongly marked in this way, which makes them, at times, reſemble a ſtring of beads. Anatom. of Abſorb. p. 65.

phæ eſt a partibus ſingulis ſuperfluum ſerum remittentibus, vel ipſis arteriis, lymphâ abundantibus. Anat. Renov. P. 723-4.

(B) Cæterum quamvis vaſa lymphatica humorem ſuum ab arteriis accipere aſſerimus, non tamen lymphaticorum cum arteriis immediatam ponimus anaſtomoſin, niſi mediantibus cribris, vel aliis corporis partibus, in quæ ubi ſanguis effuſus, partium nutritioni et humorum ſecretioni proſpexerit, vaſa lymphatica lympham, venæ ſanguinem revehunt. P. 99-100*

(C) Etenim iſta lymphatica, æque ac reliqua totius corporis, non tam a membranis provenire, ſed vel ab interſtitiis, ubi arteriæ terminantur, &c. P. 68†.

(D) Parens meus venerandus, qui primus et nomen et æternitatem dedit vaſis a ſe inventis, lymphatica ab omnibus partibus, tam glanduloſis quam extremis artubus lympham, ad ſanguinem referre credidit. Struct. Nov. Diaphrag. P. 89.

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* Caſpari Bartholini Thom. F. Diaphragmatis Structura Nova. Pariſiis, 1676.

† Johannis Van Horne, Opuſcula Anatomico-Chirurgica, cum Annotationibus, &c. ad aucta ſtudio D. Johannis Gulielmi Pauli. Lipſiæ, 1707.

(E) Vaſa lymphatica, quæ admodum dextrè in teſticulis detexit noſtri ſæculi curioſitas, non ſolum provenire nobis videntur, ſed etiam ab internâ teſticulorum ſubſtantiâ. P. 35*.

(F) Vaſa hæc lymphatica, tam interna quam externa, longe etiam et a reliquis lymphaticis diverſam vehunt lympham, utpote particulis bilioſis magis volatilibus gravidam, ideoque meritò lympham bilioſam dicendam. P. 17†.

(G) Quod etiam nobis confirmat Kirkringius, qui, quia in vaſe lymphatico, per ſummam fellis cyſtidem reptante, lympham ſubflavo colore tinctam conſpexit, et fellis ſaporem guſtu percepit, concludit lympham hanc amariorem extra arteriarum vaſa delatam fuiſſe; quia ſi vaſorum lymphaticorum cum arteriis eſſet anaſtomoſis, non amara reperitur ſed dulcis, qualis in arteriis deprehenditur. Struct. Nov. P. 100.

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* Regneri De Graaf Opera omnia. Lugd. Batav. 1677.

† Diſſertatio Anatomico-Medica de Motu Bilis Circulari ejuſque Morbis; Auct. Mauritio Van Reverhorſt, Medic. Cand. Lugd. Batav.

(H) Verum neque tu inficias iveris opinor, aliquid per cutem in vaſa capillaria tranſire poſſe, adeoque ſanguini commiſceri. Credo te (in praxi medica feliciter ac diu occupatum) fotuum uſum non raro imperaſſe: puta dum ſchirroſis tumidiſque abdominibus opem adferres, dubitaſne igitur, an aliquid eorum per cutim in vaſa ſanguifera penetret? P. 153.*

(I) Nempe crediderunt nonnulli, e partibus nutritis emergere iſta; alii glandulas omnes majores conglobatas primam omnium eorum ſcaturiginem ſalutarunt; alii hæc poſteriora negarunt, cum in partibus, pulmonibus ſcilicet, reperiuntur, ubi tamen nulla glandulosa ſtructura notata fuerit. Veritatem dicunt fere omnes. Membranas, ut glanduloſas, omnibus muſculis, verbo omnibus partibus et viſceribus ſuper extenſas, tanquam *principium*, fontem, ac *originem*, declarabimus, cum illa æque ex conglomeratis ac conglobatis, hepate, pancreate, teſtibus, viſceribus omnibus, ex artubus, abdominis lumborum, et intercoſtalium muſculis, diaphragmate, pleura, pericardio, ac meningibus,

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* De Reſpirationis Uſu primario Diatriba; Auct. Malachia Thruſton, M. D. Lugd. Batav. 1671.

ningibus, *verbo omnibus partibus egredi videamus.* P. 819*.

(K) Imo recentiorum docuit diligentia, vix ullam noſtri corporis partem, iiſdem carere; nam et in pulmonibus, mediaſtino, ventriculo, liene, teſtibus, utero, narium ſepto, palpebris, palato buccis, &c. reperiuntur. Opera Omnia. P. 73.

(L) Unde opinari licet, venas lacteas non directe et immediate in inteſtina hiare, ſed obliquè inter eorum tunicas ferri, antequam in cavitates inteſtinorum penetrent; non aliter, forſitan, ac ductus communis in duodenum, aut ureteres in veſicam terminantur. De Corde. P. 150.

(M) Inſeruntur in omnia inteſtina, in quæ oſculis ſuis hiant, mucoſæ materiæ cruſtâ oblinitis; et ne ad inteſtina verſus relabatur chylus, ſemel acceptus, donatæ ſunt valvulis. Anat. Renovat. P. 612.

(N) Exſurgunt ab inteſtinis, inter quorum tunicas plurimis tenuiſſimis radicum finibus verſus interiorem eorum capacitatem hiant, oſculis ſub ſpongioſo quodam muco latentibus. Anatome. P. 67.

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* Diſſertatio Anatomica de Vaſorum Lymphaticorum Adminiſtratione; Auct. Joh. Zellero. M. D. Tubingæ, 1687.

(O) Sunt autem vaſa (lactea) tenuia et pellucida, ſimplici tunica donata, per meſenterium diſperſa, numero infinita, chylo vehendo dicata. Anatome, P. 67.

(P) Membrana vaſculum lymphaticum componens, externo oculo examinata, pellucida eſt et tenuis, nihilque peculiare (quoad ſtructuram) ob partium tenuitatem ſenſibus externis offert. Verum microſcopio examinata, texturam præbet infinitorum globulorum, majorum minorumque, contactu mutuo inter ſe cohærentium; quorum nonnulli ductulos aliquot, ſed irregulares, arearum in modum, componere videbantur, variis aneſtomoſibus inter ſe junctos; hi autem tractus, an vaſcula fuerint, necne, pro certo, affirmare non auſim. P. 41-2*.

(Q) Nodoſa quoque ſunt, et externe apparent inæqualiter tumentia, imprimis ſi, ligatis illis lympha ſiſtatur, idque propter valvulas, non tantum ingreſſui in axillarem affixas, ſed in omnibus ramis diſpoſitas, geminataſque ubique et oblique invicem ſuperpoſitas, textura tenerrimas et numeroſiores quam in

venis,

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* Adenographia Curioſa et Uteri Fœminei Anatome Nova, Auctore Antonio Nuck, Med. Doct. Ludg. Batav. 1696.

venis, adeo ut ſupra bis mille in lymphaticis aliqui demonſtrent, quarum uſus eſt impedire ne aqua ad lymphatica regrediatur. Anat. Renov. P. 622-3.

(R) Manifeſte conſtabit, eundem ab hepate, non autem ipſum verſus tendere; idque etiam valvularum ſtructura, quæ numeroſæ in iis reperiuntur, dilucide evincit; illæ ſiquidem liquori ei hepate effluenti facile cedunt, regreſſuro autem ſecluduntur. P. 268 *.

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* Franciſci Gliſſonii, in inclyta Cantabrigiæ Academia Medicinæ Profeſſ. et Coll. Lond. Soc. Anatomia Hepatis. Ad Calcem Operis ſubjiciuntur nonnulla de Lymphæ-ductibus nuper repertis. Londini, 1654.

SECT.

SECT. V.

IDENTITY *of* LACTEALS *and* LYMPHATICS.

When Aſellius diſcovered the abſorbents of the inteſtines, he called them lacteals, from the fluids they contained; in like manner, Rudbeck and Bartholin gave the abſorbents, in other parts of the body, the name of ſerous and lymphatic veſſels, for the ſame reaſon. It was, however, found, that the lacteals ſometimes carried lymph, and, therefore, the term lymphatic was equally applicable to them. The appellation of lacteal, however, was retained for the ſame reaſon, that it ſtil continues in uſe, *viz.* to diſtinguiſh between veſſels which receive chyle from the inteſtines, and thoſe, in other parts of the body, which never contain that fluid. Notwithſtanding that this diſtinction of name remained, yet we find it mentioned, that the lacteals, occaſionally, contained the ſame fluid with the other obſorbents of the ſyſtem, and therefore, were called lymphatics. Mr. Cruikſhank obſerves, that ‘ the lymph is the fluid, ſuppoſed, by the firſt diſcoverers of the lymphatics, to be found *only* in them, and not in lacteals; and which occaſioned their forming theſe veſſels into two claſſes’*. I confeſs, that I do not find, in the Anatome Renovata of Bartholin, any expreſſion, by which he unequivocally means, that the vaſa lactea ever contained lymph; but he aſſerts, that the thoracic duct always contains lymph, except when it is diſtended with chyle. (A) The receptacle receives the lymph from lymphatics, and through the ſame thoracic lacteal ducts, delivers it to the heart. It is always filled with lymph, when no chyle is preſent†. And again, during the time of diſtribution, its chief office is to carry chyle, but always at other times lymph. In a work, however, which we have ſince met with, publiſhed by Bartholin, he aſſerts, that he has ſeen the lacteals turgid with lymph. (B) I have more than once lately ſeen, in animals, opened two hours after they had taken food, the lacteals containing

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[*] P. 95.

[†] Diemerbroeck alſo aſſerts the ſame, P. 76.

taining no chyle, but all of them paſſing from the inteſtines, turgid with the cleareſt lymph, and creeping along the glands, after the manner of the lacteals'.

Without having recourſe to any further quotations from Bartholin, I find, in a work publiſhed only a few years after the Anatome Renovata, as full and clear an account of the identity of lacteals and lymphatics, as can be expreſſed in words. Speaking of the receptaculum chyli, the author* ſays, (c) 'This ſack is formed by the union of three branches of abſorbents, *viz:* the hepatic, meſenteric, and iliac. Having mentioned the ramus iliacus, he adds; The ſecond branch is the meſenteric, the greateſt of the lacteal veſſels, *all which are true lymphatics*, and are *only lacteals during the time of diſtribution; that being paſt, they again become turgid with lymph.* He ſays, the lymphatics are found in all parts of the body. There is no part of the whole body, which does not contain

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* Zellerus. The Editor of Van Horne's Opuſcula thus ſpeaks of this author. Circa modum vaſa lymphatica obſervandi et demonſtrandi Zelleri diſſertatio meretur evolvi.

ſuch veſſels; ſo that they not only paſs out from the conglobate, but alſo from the conglomerate glands, from muſcular parts, from all the joints, from the diaphragm, the lungs, &c. but he adds, they are not every where equally numerous. Nuck, too, had long ago mentioned, the ſimilarity of ſtructure between lacteals and lymphatics; ſince therefore the ſtructure of the lacteals and lymphatic veſſels is the ſame, &c. In another place he ſays, (D) ‘ The meſentery has alſo its lymphatics, of the ſame ſtructure with the lacteals. I have ſometimes remarked, that the lacteals of the meſentery not only afforded a paſſage to the chyle, but alſo to the lymph; the diſtribution of chyle being finiſhed and received into the blood, the lacteals muſt give entrance to the lymph; for, beſides that, we have ſometimes ſeen lymph in the lacteals, it is not difficult to conceive, that the greater portion of the pancreatic juice, and alſo the lymph, ſecreted from the glands of the inteſtines, not finding any other way than theſe, by which they will therefore paſs through the thoracic duct into the blood. The ingenious Editor of Van Horne's works alſo maintains, that the lacteals ſometimes carry lymph. (E) Moreover, the lacteal veins, *primi generis*, according to Ruyſch,* have not ſo many valves as the lymphatics; and, indeed, the lacteals, *ſecundi generis*, *although both ſets are true lymphatics*, carry a mere lymph, when chylification is completed.'

No comment can ſurely be required to theſe clear expreſſions. Why did Hunter and Monro ſuffer theſe truths to paſs unnoticed? It appears then, from the teſtimony of Bartholin, Nuck, and others, that the lacteals and lymphatics were the ſame ſyſtem of veſſels, were ſimilar in ſtructure, and occaſionally carried the ſame fluids.

(A) Lympham ex lymphaticis recipit receptaculum, et per eoſdem ductus thoracicos lacteos amandat ad cor. Hoc lympha fere ſemper repletur quando chylus deeſt. P. 112. Chylum tempore diſtributionis, primario vehere et ſemper alias lympham. P. 621.

(B) Semel atque iterum nuper vidi in animalibus, duas poſt paſtum horas apertis, nimirum

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* Dilucid. Valvular. Cap. II. p. 34.

venas lacteas nullas chylo repletas, ſed omnes ab inteſtinis prodeuntes, lympha limpidiſſima turgidas, eodem more glandulas meſenterii perreptare, quo lactea conſuevere. P. 94. Struct. Diaphrag.

(C) Saccus ille formatur ex unione trium ramorum, hepatici, meſenterici, ac iliaci. Secundus ramus inſignior eſt meſentericus ex omnibus lacteis vaſis, quæ omnia vera lymphatica ſunt, et lactea diſtributionis tempore ſolum apparent, iſta vero elapſo, iterum lympha turgent. Nulla eſt pars totius corporis, quæ non talibus gaudeat; ita ut non ex glandulis ſolum conglobatis egrediantur, ſed etiam conglomeratis, ex muſculosâ parte, omnibus artubus, diaphragmate, pulmonibus, &c. P. 812-13.

(D) Meſenterium etiam ſuos habet lymphæductus, eandem fere fabricam ac vaſa lactea componentes. Verum aliquoties notavi, vaſa meſenterii lactea, non tantum chylo, ſed et cum eo lymphæ tranſitum præbere, imo peractâ chylificatione, chyloque jam in ſanguinem advecto, humori lymphatico tantum eadem patere; namque præterquam, quod lympham aliquoties in lacteis viderimus, etiam conceptu difficile non erit, ſi ſucci pancreatici portionem majorem, necnon in inteſtinorum glandulis lympham ſecretam, alias vias, præter has, nuſpiam invenire, per quas ductui thoracico, et ita ſanguini, accedere poſſet. P. 146-7.

(E) Venæ inſuper lacteæ primi generis, notante Ruyſchio, Dilucid. Valvular. Cap. II. P. 34, non tanta valvularum copia, quanta vaſa ſeroſa; imo et ſecundi generis lacteæ, quamvis utiuſque generis vaſcula revera ſint lymphatica, lympham videlicet meram alio, quam chylificationis tempore tranſportantia. P. 51. Note x.

SECT. VI.

FUNCTIONS *of the* ABSORBENT SYSTEM.

Having laid before the reader what was known in the laſt century, reſpecting the ſtructure, origin, &c. of the abſorbents, it remains, that we ſhould treat of their functions. A conſiderable part of this ſubject has been, unavoidably, anticipated, in the preceding ſections; but it is proper to bring the whole in one connected point of view, that it may be ſeen, how far our predeceſſors were acquainted with this part of phyſiology. The chyle is the fluid, which the lacteals moſt generally receive from the inteſtines, although they will, occaſionally, contain a variety of other ſubſtances. The firſt diſcoverers attributed to the lacteals, the abſorption of chyle; and, therefore, muſt alſo have allowed, that what was occaſionally mixed with chyle, would alſo be taken in. Bartholin, and others, mention, that they take up medicines,

poiſons,

poiſons, &c. Of this, indeed, there was no doubt; but it has been imagined by the moderns, that the firſt diſcoverers did not know how the lymph, &c. got into the abſorbents of the body. It was the opinion of Bartholin, Willis, Gliſſon, Zellerus, &c. that the lymphatics were not continued from arteries, by continuity of canal. How then was it imagined, that the lymph was received into their mouths? The mode of abſorption, is a ſubject which ſtill divides the phyſiological world. No one doubts, that the abſorbents are intended to receive the various ſubſtances applied to their mouths; but it is ſtill a matter of diſpute, *how* the ſubſtance gets into the abſorbent; whether by the activity of the abſorbents themſelves, or by their action as capillary tubes, or by the preſſure of ſurrounding parts. Mr. Cruikſhank, himſelf, is, by no means, decided on this point. He is undetermined, whether to accede to the opinion of Aggiunti, of Boerhaave, or of John Hunter. We muſt not be ſupriſed, then, if the authors of the laſt century had their doubts upon the ſubject.

As many ingenious men flouriſhed about the period, when the diſcovery of the lymphatics was much talked of, their great attention to medical purſuits; and to the laws of the animal œconomy, as their works fully teſtify, muſt have led them to the knowledge of the great importance of the abſorbent ſyſtem. They obſerved fluids continually pouring into different cavities; they knew, from the experiments of Bartholin, Lower, and others, that the red veins did not abſorb; they alſo knew, that the red veins did not throw out any fluid; they muſt, then, have concluded, that other veſſels exhaled, and that the lymphatics abſorbed. Accordingly we find, in the works of Gliſſon, Willis, and others, a full account of theſe important functions of the animal frame. Willis gives the following account of the lymphatics of the lungs. (A) ‘ Indeed a conſiderable number of veſſels of this kind are required in the lungs; for ſince the blood is here *chiefly heated*, and rapidly circulated, the veins do not eaſily receive all the blood from the arteries; moreover, the glands do not long retain *what is effuſed into them;* therefore there was a neceſſity for lymphatics, as ſo many rivulets, by which the continually ſuperabundant moiſture might be carried off. If it happens, that theſe veſſels are obſtructed or ruptured, an anaſarca pulmonum, or hydro-thorax will ariſe, as alſo coughs, and phthiſical affections; will be often produced*. Nuck alſo ſays, that dropſy may ariſe from rupture of lymphatic; and mentions a caſe, of what he ſays is a new diſeaſe, *viz:* a dropſy of the peritonæum.

Paulus, in his commentary on Van Horne's Microcoſmus, mentions this opinion of Nuck †; and Diemerbroeck held it as probable, that dropſy might ariſe from the ſame cauſe. (B) We muſt obſerve, with reſpect to the lymphatics of the abdomen, that if they be ruptured from any cauſe (for they are of a delicate ſtructure) then the ſerous lymph will be effuſed into the cavity of the abdomen, and aſcites will be at length, produced, by its accumulation; although it alſo ariſes from other cauſes. Hydatids, he thinks, are cauſed, by the effuſion of lymph, forming veſicles. Lower doubted very much, whether the rupture of lymphatics can produce dropſy. He paſt ligatures round the vena cava of a dog; the wound was then ſewed up. The dog died in a few hours. When diſſected, a great quantity of ſerum was found in the abdomen. He alſo tied up the jugulars in another dog. All the parts above the ligature ſwelled greatly, in a few hours; and in two days, the dog periſhed, as if ſuffocated by angina *. (C) During this time, there was not only a profuſion of tears, but alſo a great flow of ſaliva from the mouth, as if it had been brought on by mercury. After death, I ſeparated the ſkin from the tumefied parts, and expected to have found them turgid with blood; but it was quite the contrary, for I could not diſcover any trace or colour of blood; all the muſcles and glands were diſtended with limpid ſerum, and appeared, in ſome meaſure, pellucid.

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* This paſſage is very remarkable, becauſe Willis ſays, that the lymph is taken up from glands: and Mr. Cruikſhank, p. 175, from ſome overſight, gives a paſſage from this author, immediately before the one we have quoted, and another immediately after, but paſſes over the very pointed expreſſions mentioned above.

† Vide Nuck's Adenograph, p. 97-98-100. Van Horne's Microcoſ. p. 57, note h.

How far theſe circumſtances tend to inveſtigate the cauſe of aſcites and anaſarca, I leave to others

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* Vide Baglivi Diſſert. de Experim. Anatom. Pract. P. 107.

to determine; I ſhall only make this remark, ſays Lower, that aſcites does not always, if ever, ariſe from ruptured lymphatics: for I have diſſected many ſheep, killed by the dropſy of thorax and abdomen, in which I found the lymphatics more turgid and full, than I ever remember to have ſeen; ſo that if any one intends to give a complete hiſtory of the lymphatics, no ſubject can anſwer his intention or deſign better. This idea of rupture, as the cauſe of dropſy, is alſo mentioned by Bartholin, and has been ſince adopted by Whytt and Cullen. Willis, again, mentions the pulmonary lymphatics, and gives a plate of them, taken from the lungs of an ox *. The following paſſage, concerning their uſe, is ſo exactly ſimilar to Dr. Hunter's theory, that I ſhall be excuſed from tranſcribing the whole of it. (D) If I dare conjecture, concerning the uſe of theſe veſſels, it is very probable, that thoſe cavities, intercepting each of the lobules, receive vapors every where, copiouſly flowing from the heated blood (particularly as they can no where be effuſed or collected with greater convenience), that is

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* Rudbeck, Bartholin, and Ruyſch, had ſeen the lymphatics of the lungs. Vide, Diemerbroeck, P. 87.

is to ſay, theſe *vapours tranſude from the terminations of the veſſels*, through their very delicate coats, and thence being propelled further, *are condenſed into water*, to be carried off from the lungs by theſe veſſels, fitted for that purpoſe: moreover, leaſt the lymph, thus formed and condenſed, from the vapours within theſe ducts, ſhould regurgitate into the lungs, they are beſet with numerous valves*.

The plates give the exact reticular appearance of the pulmonary abſorbents, as deſcribed by Mr. Cruikſhank †. In the works of Gliſſon, we find, completely, the modern doctrine of tranſudation, and abſorption. This will appear, by a tranſcript from his book. He firſt aſſerts, that the lymphatics take up a fluid, from parts into which they are inſerted. He then enquires, whence this fluid is derived. (E) Whence is this fluid derived, which thoſe veſſels carry off? It is poſſible, that the arteries may claim to themſelves, ſome part of this office. For it cannot otherwiſe happen, but that the blood paſſing through them, ſhould pour out a copious halitus upon thoſe parts, into which it is ſent; and the more ſo, becauſe the ſame halitus is, for ſome time, confined, by the thick and denſe coat of the artery. This halitus, thus effuſed, is retained, in conſiderable quantity, by fibrous and membranous parts, and is again collected; and, after the manner of diſtillation, is condenſed into a certain liquid, which forms ſome portion of the fluid, carried off by the lymphatics. He thinks, the arteries alone do not furniſh this halitus, but that it is ſupplied by other veſſels. The whole of the fluid carried off by the lymphatics, is not derived ſolely from the halitus of the blood, nor from the arteries, but alſo from other veſſels; theſe, he ſays, are nerves. But notwithſtanding that the nerves ſupplied a fluid to be abſorbed, yet he attributes the greater quantity of tranſudation to the arteries; by far the greateſt part of this lymph is condenſed from the halitus of the blood, and carried off by the lymphatics, If Gliſſon had not already expreſsly aſſerted, that a halitus is thrown out, condenſed, and taken up by the abſorbents, we might collect, from theſe paſſages, that he could not have imagined, the lymphatics to be a continuation of arteries; becauſe, he ſays, it takes a great deal of pains to prove it, that the nerves throw out a fluid to be abſorbed; which he certainly could not have ſaid, had he maintained, that the lymphatics were returned from arteries, by continuity of canal. The abſorption of ſolids was, alſo, maintained by this author. He urges, when ſpeaking of the lymph, that the abſorbents are thoſe veſſels, which return into the ſyſtem, effuſed fluids; that this effuſion is not excrementitious; for ſo true and œconomical is nature in her operations, that ſhe never leaves her work incomplete. We muſt conclude, that he meant the ſolids were taken up, from the following expreſſions. The chyle is the daily ſupport, by which the waſte of the blood is continually repaired; from its deficiency, the very ſolid parts are broken down into a fluid, and when neceſſity requires, reſtores the loſs of the blood. I could point out, were it neceſſary, numerous other paſſages in Gliſſon, to the ſame purpoſe; and if any one will be at the pains of reading Chap. 45 of the Anatomia Hepatis, his trouble will be amply repaid.

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* It is rather curious, that Mr. Cruikſhank quotes another paſſage from Willis, immediately after this clear account, without taking any notice of it.

† P. 84. During a lecture, which we heard in London, ſome time ago, on this ſubject, a plate of the lymphatics of the lungs was handed round; we were ſtruck with its exact ſimilarity to that of Willis's, and took the liberty of mentioning it after lecture.

During the latter period of the ſeventeenth century, medical men were very active, all over Europe, and eminently ſo in the learned Univerſities of Cambridge and Oxford. In their writings, we find the brightneſs of genius flowing through every page. The works of Willis, Mayow, Gliſſon, Bartholin, and innumerable others, contain experiments and obſervations, which will juſtly ſurpriſe a modern reader; and, for my part, I muſt confeſs, that in the theories of medicine, and their application to practice, I have derived conſiderable advantage, from the peruſal of their works: but, independent of theory, which the prolific brain of any may form, without a minute knowledge of anatomy, we find in them nice points determined, which require laborious anatomical inveſtigation. In the arts and ſciences, if we did not profit by the accumulated labours of our fore-fathers, our exertions would be vain, in as much as we ſhould not advance the progreſs of knowledge. Dr. Hunter ſays, he has often found his obſervations in old books, long after he had thought them peculiar to himſelf. His excellent remarks were, the reſult of tedious inveſtigation. If he had been, at firſt, acquainted with theſe obſervations, might they not have been a clue, by which he might have directed his uſeful labours? Much time would be ſaved, of that ſhort period allotted to man; and thus the various inveſtigations of many, would, as it were, be concentrated by the labours of an individual, and be productive of reſearches beneficial to ſociety*. It is ſurpriſing, that the uſeful labours of many, who have gone before us, ſhould have been ſo much neglected. Mr. Cruikſhank, through the whole of his work, takes no notice of the clear account Gliſſon gives of tranſudation and abſorption. He only quotes him once, when mentioning Jolyffe's right to the diſcovery of the lymphatics. 'As to Gliſſon's evidence I muſt obſerve, that Dr. Jolyffe conſidered the nerves as veſſels, and therefore uſes the words *quartum genus vaſorum.* At the ſame time, he appears to me to have forgot Aſellius's diſcovery of the lacteals, or he would have ſaid, *quintum genus*, unleſs he imagined his veſſels and Aſellius's the ſame; and then Gliſſon would never have ſeriouſly informed us, that Jolyffe had diſcovered a new ſet of veſſels, Aſellius having informed us of them long before'*. Gliſſon ſeems to have conſidered the lacteals and lymphatics, as the ſame ſyſtem of veſſels; for he ſays, (G) Hitherto, only four ſets of veſſels have been diſcovered, *viz.* arteries, veins, nerves, and lymphatics. He could not have forgot the diſcovery of Aſellius; for he obſerves, When I firſt ſaw theſe veſſels, the aſſertion of Aſellius, that he had diſcovered lacteals going to the liver, occurred to me†.

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* Though we allow him a great ſhare of ingenuity, ſagacity, and diligence, we find much to reprehend, and various occaſions to wiſh his knowledge more matured, by previous inveſtigations of the labours of others in the ſame departments.—Crit. Rev. for Auguſt, Art. Foot's Life of J. Hunter.

Upon conſideration it appears, that both Jolyffe and Gliſſon ranked the venæ lacteæ with red veins; and, therefore, the diſcovery of Jolyffe

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[*] P. 35.

[†] We may conclude, from this, that Aſellius ſaw the lymphatics.

would be a *quartum genus vaſorum.* We wiſh, that that diſtinguiſhed anatomiſt, Mr. Cruikſhank, had paid more reſpect to Gliſſon, and had given him that place in his publication, which his great merit and true theory certainly deſerved. No wonder that Dr. Hunter was mortified, when he read the works of Gliſſon, and was obliged to confeſs, that his theory was Gliſſon's, revived and confirmed. He might, alſo, have ſeen, that Willis believed in tranſudation. I muſt obſerve, however, that Willis, and others, did not ſeem to be aware, that tranſudation took place, only, through organic pores. They have no where poſitively aſſerted, *how* this tranſudation was performed; and as far as I know, this was left to be proved, by the ingenious author of the Treatiſe on the Abſorbent Syſtem. His excellent account, in the firſt part of his publication, forms a beautiful and conſiſtent theory. Let us now examine, what ideas were entertained, with reſpect to *how* abſorption was performed. Aſellius aſcribed the abſorption of chyle, to a traction in the heart; and this opinion was adopted by anatomiſts: but when the circulation was explained by Harvey, this

doctrine,

doctrine, in a great meaſure, fell to the ground; but even after that noble diſcovery, ſo ſtrong was prejudice, that many adhered to the old opinion of a valida tractio in the heart. I have mentioned, that Lower imagined, from his experiments, that the lacteals aroſe, obliquely, from the inteſtines*. (H) From this circumſtance, he thought, that the action of the inteſtines compreſſed the mouths of the lacteals, and prevented abſorption; but as the periſtaltic motion is not inceſſant, he concluded, that the chyle was received into the lacteals, when the motion ceaſed. It is evident, from this, he aſcribed an activity to the mouths of theſe veſſels. The chyle, having got into the lacteal, is propelled, by the vermicular motion of the inteſtines into the receptaculum. The aſcent to the chyle, would be very difficult, ſays Lower, were the receptacle not placed between the tendons of the diaphragm; whence it happens, that as it is contracted during reſpiration, its tendons being drawn cloſe together, firmly compreſs, and agitate this lacteal ſack, eſpecially when filled with chyle, ſo that they propel

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* This was alſo the opinion of Drake.

the chyle into the ſubclavian veins. Other authors, if we are to judge from the terms they uſe, attributed an activity to the mouths of theſe veſſels; for we meet with *exſugo*, *abſorbeo*, and *exhaurio*.

An opinion, however, which prevailed very much, with reſpect to abſorption, was, that the contents of the abſorbents were received into them, and propelled by the action of ſurrounding parts; making the abſorbents altogether paſſive in their functions. This doctrine is mentioned by a variety of authors. Diemerbroeck ſays, that the chyle and lymph are impelled into the abſorbents, by the preſſure of adjacent parts, as muſcular motion. (I) The cauſe of the entrance of the chyle into the lacteals, and its motion through them, is two-fold, *viz.* the periſtaltic motion of the inteſtines, and the contraction of the abdominal muſcles, together with the action of the reſpiratory organs. When ſpeaking of the lymphatics, he obſerves, (K) The impulſive cauſe is the ſame, *viz.* motion and preſſure. The lymph, which comes from the joints, is impelled by the motion of the muſcles of thoſe parts, in like manner as we ſee the ſalival liquor flows copiouſly into the mouth, by the motion of the jaw; but when we are at reſt, as in ſleep, there is little or none. By this preſſure of parts, not only the glands lying in them, but alſo the lymphatics, are preſſed upon, both by the muſcles, and the ſuperincumbent viſcera; and thus the contained fluid is ſqueezed out, and propelled forwards. I think the lymph moves ſlow and quick, at different times, according to the greater or leſs motion of thoſe parts, in which the conglobate glands and lymphatics are ſituated, as happens to the ſublingual ſalivary ducts, which proceed from conglomerate glands.

We have already mentioned, that Bartholin ſaid, the lymph was thrown out from the arteries. Diemerbroeck is of the ſame opinion. (L) Gliſſon of opinion, that the lymph is derived from the nerves; Bartholin maintains it comes from the arteries: the former idea is abſurd; the latter is the moſt probable, on account of the quantity of lymph, which cannot be ſecreted, in ſuch quantity, from any veſſels, as from the arteries. Diemerbroeck ſeems, here, to have forgot, that Gliſſon, although he held it probable, that a fluid might be derived from the nerves, maintained, that by far the greater part of the lymph was ſecreted from the arteries. Arterial action, too, is expreſsly mentioned by ſome, as a cauſe of the motion of the lymph. Bartholin, ſpeaking of the lymphatics which accompany blood-veſſels, obſerves, (M) This I ſuſpect, to accelerate the motion of the lymph, in like manner as I believe, the blood in a vein to be propelled, by the pulſation of the artery in the neighbourhood.

The doctrine, that the abſorbents are perfectly paſſive in their functions, is fully explained by Collins, an ingenious anatomiſt, who flouriſhed about the middle of the laſt century. Now ſomewhat may be ſaid of the manner, how this milky liquor is tranſmitted through the meſentery, into the common receptacle; and this, I humbly conceit, may be accompliſhed by a double means. The firſt may be, the gentle contraction of the guts (made by carnous fibres) in their periſtaltic motion, wherein the chyle, impregnated and diluted by more fluid, and excellent particles of the pancreatic juice, is impelled into the origins or roots of the milky veſſels, whoſe pores hold an analogy, in ſhape and ſize, with the particles of the chyle, and thereby give a reception to this ſelect alimentary liquor. The ſecond way of tranſmitting chyle, through the meſentery into the common receptacle, is more powerful, and aſſiſtant to the former, and are the muſcles of the belly, which contract themſelves in expiration, and thereby compreſs the guts, and ſqueeſe the purer parts of the prepared aliment into the orifices of the lacteal veſſels; and the more groſs excrements, the reliques of concoction, having magnitudes and figures, different from the orifices or extremities of the milky veins, are ſecluded their cavities, and are protruded from one part of the guts to the other, as diſſerviceable to nutrition*.

As Collins thus allowed no action in the lacteal itſelf, it was neceſſary, that ſome other power ſhould propel the chyle, once received into it. He thus deſcribes it. The chyle being thus generated in the ſtomach, is thence tranſmitted into the inteſtines, and afterwards received into the origins of the lacteal, by the periſtaltic motion of the guts, and the contraction of the abdominal muſcles, ſqueeſing the chyle into the milky veſſels, which import it through the meſentery, into the common receptacle (being lodged between the proceſſes of the diaphragm) compreſſed, when the medriff is moved, and brought towards a plain; whereupon the milky humour is impelled farther and farther upwards, through the thoracic ducts, till it landeth in the ſubclavian vein. This aſcent of the chyle is much aſſiſted by the mechaniſm of the ducts, as furniſhed with valves, which give way to the aſcent of the chyle upward, toward the ſubclavian vein, and give a check to its retrograde motion toward the common receptacle*. It is worthy of remark, that this author, through the whole of his work, never makes uſe of the word abſorption, which implies action in the veſſels themſelves; but when he wiſhes to expreſs this function, ſays, the fluid is impelled, received, tranſmitted, &c. into the origin of the lymphatic or lacteal. This anatomiſt has given the ſame theory of the motion of the lymph. The thinner ſerous excrements, are tranſmitted into the origin of the lymphæ-ducts, appertaining to the muſcular parts, by whoſe local and voluntary motion, the natural progreſs of the lympha is much quickened. Again; the lympha of the adjoining lymphæ-ducts is promoted by muſcular motion, from the circumference to the center to the common recaptacle*.

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* P. 389, Vol. I.

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* P. 682, Vol. II.

The doctrine of Collins, then, implied no muſcularity in the abſorbents, and pointed out, that they were paſſive in their office, their contents being propelled by the action of ſurrounding parts. The opinion, that the chyle was received into the lacteals by muſcular motion, found its advocates in Drake and Verheyen. The chyme, ſays the former, which, as ſoon as it is reduced to a conſiſtence, looſe enough to be obedient to the preſſure and periſtaltic motion of the inteſtines, is gradually worked out, through the pylorus, into the duodenum, along the ſides of which, and the reſt of the inteſtina tenuia, the vaſa lactea are planted, into whoſe extreme minute orifices, the thinner and more fluid part of this maſs is preſſed, by the natural contraction of the guts†.

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* P. 443, Vol. I. † P. 88.

A doctrine, preciſely ſimilar to this, has been adopted, of late, by ſome very ingenious men. Experiments and arguments have been brought, to refute the opinion of Cruikſhank, and others, who aſcribe muſcularity to the abſorbents. A very ingenious and eminent lecturer in London adheres to this opinion, denies altogether any neceſſity for muſcularity in the abſorbents, and maintains, that their contents are tranſmitted into them, and propelled by the motion of ſurrounding parts, as muſcular and arterial action. I muſt confeſs, it is difficult to account for the rapid abſorption of fluids, in many caſes, without allowing ſome muſcularity in theſe veſſels; at the ſame time, the irritability and muſcular power attributed to them by many, is much too great, and let me add, incompatible with the ſafety of life*. We cannot admit of the great irritability attributed by Mr. Cruikſhank to the abſorbents, although we think they poſſeſs ſome portion of muſcular power. If I am not miſtaken, and if we are to judge from his expreſſions, he gives them an irritability, greater than that of the heart. It has been imagined, that the veſſels continue to abſorb after death. This has been brought forward as one of the moſt powerful arguments, in proof of their irritability. The muſcular fleſh of the turtle, ſays Mr. Cruikſhank, continues to contract and relax, from the ſtimulus of air only, many hours after the animal's head is cut off. The abſorbents appear to me, to have a ſimilar power of action, and to be capable of abſorbing, for ſome time after the animal is dead. Malpighi had before ſaid, that one would be tempted to believe, that they abſorbed after death, and I was determined to make the experiment. I tied up the trunks of the arteries and veins, belonging to a portion of the great inteſtine in an aſs, which had been dead a few minutes (the inteſtines were ſtill in the cavity of the abdomen and the parts not cold). I knew, that the trunks of the abſorbents muſt be incloſed in the ligature, though not one of them was then viſible. Two hours after, I returned, and found a number of abſorbents, turgid with a tranſparent fluid. I opened one of the largeſt with a lancet; the fluid iſſued in a ſtream, which it could not have done, unleſs the veſſels had continued to abſorb and to propel their fluids, with great force, after the death of the ſyſtem *. This circumſtance was before taken notice of by anatomiſts; but it was accounted for in a quite different manner, and upon principles more agreeable to truth. The ingenious Verheyen has the following obſervations. (N) The motion of the lymph does not ceaſe with life, like that of the blood, but continues a conſiderable time after death, which is proved, by ligatures being paſt round the lymphatic veſſels. This ſeems to ariſe from the following circumſtances: 1ſt. Becauſe the lymph is very fluid and ſmooth, and therefore flows eaſily. 2d. Becauſe the veſſels themſelves are lined with a coat, extremely poliſhed, which ſuffer compreſſion from the collapſed parts, and therefore the lymph is propelled forward, according to the natural motion. The proof, too, given, of their muſcularity, is very fallacious; becauſe motion was perceived

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* Mr. J. Hunter ſays, they devour parts, as caterpillars do leaves. A very late writer talks of their *pulling down*, and carrying away the materials, that are worn out, and unfit for the ſupport of the machine.

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* P. 60-1.

perceived when oil of vitriol was applied, it has been concluded by Haller and Cruikſhank, that they are extremely irritable. Every one knows, that the cauſticity produces a chemical change; it corrugates the lacteal, hence the fluid is propelled*. I have applied ſulphuric acid to the inteſtines of mice, after death, and found, that not only motion took place, but that a hardneſs was induced. This is more evident in frogs; their inteſtines are beautifully tranſparent, white, and ſoft; by applying ſulphuric acid, they become opake and firm, very ſimilar to the tunica albuginea of the eye. We have ſeparated the coats of the ſtomach of a frog, in this way; for the acid corrugates the external coat, and the internal eaſily ſeparates: it corrugates, equally, the ſkin †, the lungs, the inteſtines, &c. Hence, we conclude, that it is not an unequivocal proof of the muſcularity of parts; a mechanical ſtimulus would be a better teſt ‡.

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* An experiment, ſimilar to this, was made by an anatomiſt of London, with the lacteals.

† The ſkin of a frog is perfectly membranous.

‡ Hanc arteriarum actionem ſatis demonſtrant ipſius arteriæ

From what has been ſaid, I think, no one can doubt, but that the great importance of the abſorbent ſyſtem was ſeen and admired by the medical world, during the laſt century. The many eminent men, who then flouriſhed, have expreſſed their ideas, on the ſubject, in ſuch clear and decided language, they have proved their opinions by experiments and arguments ſo complete, as cannot fail to bring conviction home to the moſt ſceptical mind. Their attention to the ſubject is evident, through all their works; and their accuracy is confirmed by modern labour and inveſtigation. The candid enquirer after truth, is ſurpriſed, how expreſſions, ſo clear, could be miſtaken; and the benevolent philoſopher hopes, that overſight, not wilful miſrepreſentation, has cauſed this want of candour. Highly as we reſpect the great abilities of Dr. Hunter, indebted as all are to him for his extenſive and uſeful purſuits, yet ſurely it is worſe than prejudice, to adhere blindly to the *ipſe dixit*

of

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riæ fabrica, manifeſte muſculoſa tunica inſtructæ et multa quoque experimenta in vivis animalibus inſtituta; ubi arteriæ, *adhibito acri mechanico (nam veneno chemico parum fidendum eſt)* validiſſime ſe contraxerunt.—Conſpect. Medicinæ, P. 235, Vol I.

of ſuperior merit. How long, and how much, has not ſcience, and medicine in particular, ſuffered, from ſuch unphiloſophical bigotry.

'*Nullius addictus jurare in verba magiſtri*';— is a maxim, which, if not licentiouſly adopted, leads to calm enquiry, and would, in a great meaſure, have ſhortened the dark and tedious period of Galenic attachment. To acquieſce, implicitly, in the opinions of a man, becauſe he has gained applauſe and reputation, is to ſhut the door againſt inveſtigation and enquiry; and the doctrines of a teacher are only to be held in veneration, as they are found to be conſonant with reaſon and truth. It is not the authority, but the truth, which carries reſpectability along with it; and authority, if not guided by judgment, eventually obſtructs, inſtead of promoting the great end of ſcience; and the man, who propoſes a doctrine, tacitly confeſſes, that every one who reads it, has a right to form their own judgment. 'Non enim tam auctoritatis in diſputando, quam rationis momenta quærenda ſunt. Quinctiam obeſt plerumque iis, qui diſcere volunt, auctoritas eorum, qui ſe docere profitentur: deſinunt enim ſuum judicium adhibere; id habent ratum, quod ab eo quem probant, judicatum vident.'* Such was the philoſophical creed of Cicero, and ſuch ought to be the opinion of every enlightened mind.

Sydenham, by adhering to no ſect, but by carefully attending to the operations of nature, has acquired that reputation in the literary world, which will be always attached to his name; and had the great Hippocrates confined himſelf to the doctrines of Pythagoras, thoſe noble aphoriſms, which have immortalized his name, would never have adorned and improved the ſcience of medicine.

If the claims of Dr. Hunter be founded in juſtice, the opinions we have advanced, will contribute to eſtabliſh that right; for if the paſſages we have ſelected ſhall induce medical men to examine the works themſelves, they will then ſee, whether the concluſions we have drawn be inconſiſtent with the premiſes; for it is a maxim, unerring in its application, that a calm and diſpaſſionate inveſtigation always promotes the knowledge of truth. Quotations are but feeble proofs, when compared to reading the works themſelves, where detached paſſages are connected by cloſe reaſoning, and accompanied by an accurate detail of facts.

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* De Natura Deorum, P. 11 12.

The numerous quotations which appear in this miſcellaneous work were unavoidable, from the nature of the ſubject. Merely to have made aſſertions, without adducing proofs, would have been little regarded; and had I given a tranſlation of the paſſages, without the original, it might have been urged againſt me, as my own interpretation. As it is, I fear, ſome will regard it as dry reading, to wade through a maſs of quotations. Such, I may ſafely affirm, are equally as careleſs upon whom the fate of ſcience devolves, as from whom the merit of a diſcovery is injuriouſly wreſted.

(A) Profecto vaſa hujus generis quam plurima in pulmonibus requiruntur; quippe cum ſanguis hic maxime efflagret, et rapidè circuletur, venæ haud facile totum cruorem ab arteriis excipiunt; porro glandulæ quod in iis depoſitum eſt non diu continent, quare lymphæ-ductus, quaſi alveis per-

multis, opus erat, quibus humor continuo ſuperabundans amandari poſſit. Si quando hos obſtrui aut diſrumpi contingat, pulmonum aut pectoris hydrops, quinimo tuſſis et affectus phthiſici, ſæpenumero oriuntur. P. 19.*

(B) Notandum, de vaſis lymphaticis in inferiore ventre latentibus, quod ſi aliqua de cauſa rumpantur (ſunt enim valde tenella) tunc ſeroſam lympham in abdominis capacitatem effundi, ab ejuſque accumulatione, tandem aſcitem generari; quamvis aliis de cauſis fieri poſſit. P. 95.

(C) Toto hoc tempore, non ſolum lacrymæ copioſius fluebant, ſed et plurima ſaliva ex ore, non aliter quam ſi ab aſſumpto mercurio fluxus ille concitatus fuiſſet. Poſt obitum, cutem ejus a partibus tumefactis ſeparavi, atque expectavi ut partes tumefactæ ſanguine extravaſato turgerent; ſed aliter omnino evenit, quippe nullum veſtigium aut colorem fere ſanguinis obſervare potui: ſed muſculi omnes et glandulæ ſero limpido maxime diſtentæ et admodum pellucidæ apparebant. Quan-

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* Pharmaceutice Rationalis ſive Diatriba de Medicamentorum Operationibus in Humano Corpore. Pars Secunda. Auct. Thō. Willis, M. D. in Univ. Oxon. Prof. Sedleiano.

tum hæc ad aſcitis et anaſarcæ cauſas inveſtigandas conducant, aliis judicandum relinquo; hoc ſolum advertere oportet, aſcitem non ex ruptis lymphaticis ſemper, ſi omnino unquam evenire; quippe plurimas oves, hydrope pectoris atque abdominis defunctas, diſſecui, in quibus vaſa lymphatica ita turgida et repleta ubique inveni, ut nuſquam magis; adeo ut ſi quis venarum lymphaticarum hiſtoriam plene abſolvere in animo habeat, nulla corpora ad votum ejus et propoſitum magis ceſſerint. P. 82-3.

(D) Circa horum uſus ut conjectari auſim, veriſimile eſt cavitates iſtas, quoſque lobulos intercipientes, vapores a ſanguine accenſo quaquaverſus copioſe emanantes, (cum nullabi melius detrudi aut ſeponi poſſint) excipere, qui ſc. e vaſorum finibus in cavitates haſce, per tenuiſſimos ipſorum parietes, tranſudant, indeque mox ulterius propulſi, in aquam, per hæc vaſa appropriata e pulmone deportandam, condenſantur: inſuper ne lymphæ intra ductus iſtos, a vaporibus factæ ac ita condenſatæ, in pulmones (magnam ipſis noxam illaturæ) regurgitent, denſiſſimi valvularum obices impediunt. P. 32.

(E) Quid humorem illum apportet, quem vaſa iſthæc auferunt? Poſſunt quidem arteriæ indirecte et veluti per accidens, atque aliquatenus muneris hujus partem aliquam ſibi vindicare. Fieri enim nequit, quin ſanguis per illas exiliens (utpote calore inſigni effervefcens) copioſos halitus diffundat in partes illas, in quas immittitur: eoque magis, quod iidem halitus jam antea a craſſa denſaque arteriæ tunicâ aliquandiu repreſſi fuerint. Halitus iſti, ſic diſperſi, a partibus fibroſis ac membranoſis, magnam partem retinentur et recolliguntur denuo, atque ad diſtillationis modum, in liquorem quendam condenſantur, qui humoris a lymphæ-ductibus avecti partem aliquam conſtituit. Materiam omnem per lymphæ-ductus delatam, non ſolum a ſanguinis halitibus proficiſci; neque ab arteriis duntaxat, ſed et ab aliis quoque vaſis ſuppeditari. P. 402-3. Partem hujus lymphæ longè maximam a ſanguinis halitibus, roris inſtar coactis, et per lymphæ-ductus refluis, conſtitui. P. 448.

(F) Chylus eſt victus quotidianus, quo ſanguinis diſpendia reparantur denuo, in illius autem defectu, ipſæ ſolidæ partes reſolvuntur et in liquorem abeunt atque ubi neceſſitas ingruit, ſanguinis jacturam reficiunt. P. 410.

(G) Hactenus quidem quatuor duntaxat vaſorum communium genera innotuerunt, arteriæ ſcil. venæ, nervi, et lymphæ-ductus, P. 403. Cum primum vaſa hæc conſpicatus eſſem, ſubiit animum Aſellii aſſertio, quâ ait, exploratum ſibi eſſe venas lacteas in hepar diſtribui. P. 268.

(H) Probabile eſt, tunc temporis chylum in venas iſtas aſſumi, quando pars aliqua inteſtina a motu et corrugatione conquieſcit, et proinde orificia lactearum laxiora et patentiora fiunt, P. 150. Et quandoquidem chylus per vaſa thoracica, præſertim in erecto corporis ſitu, ut in homine, difficilius aſcendit, et proinde ob motus tarditatem ad coagulandum proclivior eſt; ideo inter tendines diaphragmatis, ubi ſpinæ alligantur, magnum hoc receptaculum conſtituitur; quo fit ut quoties diaphragma in omni inſpiratione contrahitur, tendines iſti valde attracti, ſacculum hunc lacteum modo chylo turgidus fuerit, plurimum comprimant et exagitent; adeoque chylum in illo contentum, per ductus chyliferos, in venam ſubclaviam propellant, et continuo vectigali maſſam ſanguinis continuo pereuntem inſtaurent. P. 152-3.

(I) Cauſa quod chylus lactea vaſa ingreditur, per eaque moveatur, duplex eſt; una debilior, ſcilicet undoſa contractio quæ a fibris ipſorum inteſtinorum peragitur, quam valde conſpicuum exhibent cuniculi et feles vivi ſecti. Altera fortior muſculorum abdominis una cum reſpiratione ſurſum deorſumque motorum impulſio. P. 69.

(K) Cauſa enim impellens eadem eſt, ſcilicet, motus et preſſio. Quæ ex artubus progreditur, per motum muſculorum iſtarum partium promovetur, eodem modo ſicut videmus per motum maxillæ etiam ſalivalem liquorem copioſum in os effluere, per quietem vero et in ſomno, paucum vel nullum. Hac enim partium preſſione, ſimul etiam in iis latentes glandulæ, ut et vaſa lymphatica, cum a muſculis, tum a depreſſis incumbentibus viſceribus premuntur, ex iiſque contentus liquor exprimitur et propellitur. Ego exiſtimo lympham modo citius, modo tardius moveri, pro majore minoreve motu ac preſſione illarum partium, in quibus ſubſiſtunt glandulæ conglobatæ et vaſa lymphatica, ſicut illud ipſum contingit in vaſis ſalivalibus ſublingualibus, quæ ex glandulis conglomeratis procedunt. P. 94-5.

(L) Gliſſonius e nervis, Bartholinus ex arteriis, eam (lympham) prodire exiſtimat. Prius abſurdum eſt. Poſterius, propter lymphæ copiam (quæ a nullis vaſis copioſior ſecerni poteſt, quam ex arteriis) maxime veroſimile videtur. P. 94.

(M) Quod motum lymphæ accelerare poſſe ſuſpicor, non ſecus ac ſanguinem in vena per arteriæ illi junctæ pulſus propelli credidi. Diaphragm. Struct. P. 101.

(N) Tranſitus lymphæ non, uti ſanguinis, cum vita ceſſat; ſed etiam notabili poſt mortem tempore perſeverat, quod probatur, per ligaturum vaſis lymphaticis injectam. Rationem puto, tum quod lympha tenuis ſit et multum fluida, adeoque ad motum valde apta; tum quod ipſa vaſa conſtent tunica admodum tenui, quæ proinde ceſſante poſt mortem calore naturali, a partibus collapſis facile comprimitur, et quidem magis in partibus, quæ poſita ligatura exteriora reſpiciunt; ſic ut lympha alioquin in viis hæſura, promoveatur continuo verſus interiora, conformiter ad propulſionem naturalem. P. 197-8.

APPENDIX

CONTAINING A

CORRESPONDENCE

WITH THE

AUTHOR,

RELATIVE TO

SEVERAL SUBJECTS
IN THE WORK.

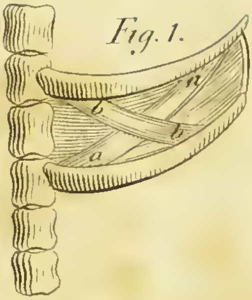


Fig. 1.

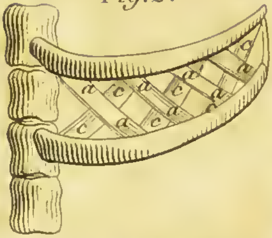


Fig. 2.

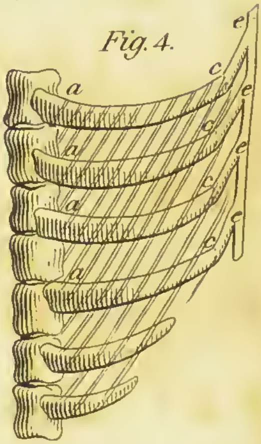
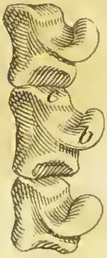


Fig. 4.



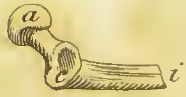


Fig 3.

Fig.1.

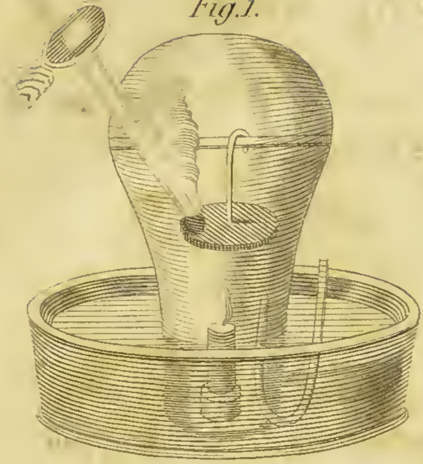


Fig.4.

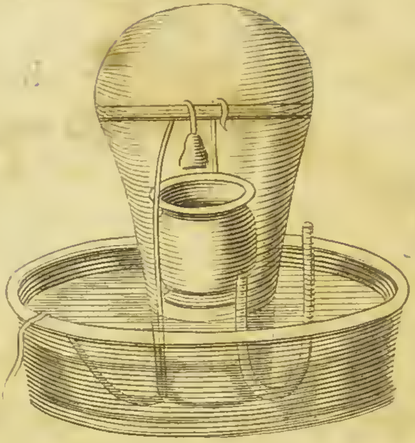


Fig.2.

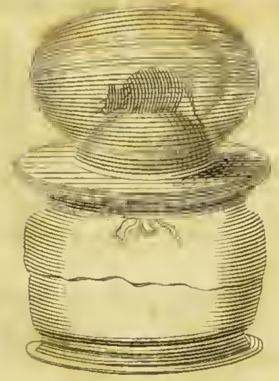


Fig. 5.

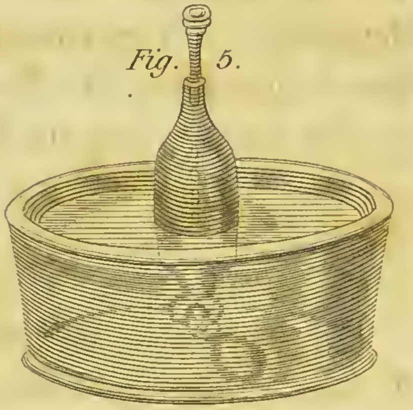


Fig.3.

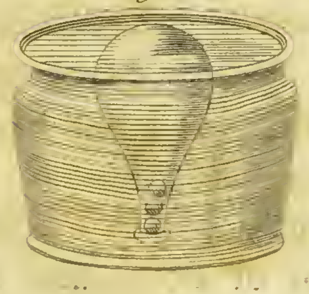
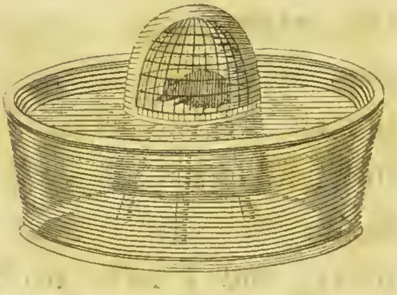


Fig.6.



APPENDIX, No. I.

Letter *from Dr.* Beddoes, *concerning an Experiment made by* Mayow.

In Sect. VI. P. 100, is related a curious experiment, on transfuſion, mentioned by Mayow. Having read the works of this author, without detecting the experiment, and having afterwards ſeen it quoted by Dr. Beddoes, we wrote to him on the ſubject, and the following is the anſwer we received.

Dear Sir, *Clifton, Dec.* 5, 1797.

I am much obliged, by your polite letter. I was, at the ſame time, a good deal alarmed at your ſaying, that you could not find the remarkable experiments on transfuſion. I have looked over the chapter, to which I refer, but without ſucceſs. I ſhould have felt ſure that I had ſhewn the paſſage to ſeveral individuals; but at the diſtance of eight years, I will not truſt my memory; and I have no means of aſcertaining the truth. If I have any papers, by help of which I could clear up the miſtake, they are not now at my diſpoſal, having been packed up, when I quitted Oxford, and left at one hundred miles diſtance from Clifton.

I have been long ſenſible, that my abſtract of Mayow was drawn up with more ardour than judgment. I ſhould have referred, for example, to his tracts, page by page. But I had no conception, that I could attribute to him an experiment, which he never made, and one ſo remarkable in its plan and reſult. Perhaps your reſearches may enable you to explain the myſtery. If there be not in Mayow any paſſage, which I could have miſtranſlated, have you met with no author, whom I might have had upon the table, and in the moment of diſtraction, have quoted in his ſtead? Have you Dr. Schereris' Analyſis? It is in German; I know not the title, but it is, I dare ſay, very accurate. I ſhould be glad, if I did commit ſo enormous a blunder, to give you and the Public all the ſatisfaction in my power, by accounting for it; but, at preſent, I am utterly unable. If you chooſe to inſert the whole, or any part of the above in your publication, it is much at your ſervice. Should any thing more ſatisfactory occur, I will trouble you with a few lines. I am glad you have ſucceeded with the foſſil alkali, in irritation of the bladder:* it has pretty faithfully ſerved me in ſuch caſes.

I am,

Dear Sir,

Your's, &c.

To Dr. Yeats,
Bedford.

THOMAS BEDDOES.

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* This was a caſe of very long ſtanding, and had reſiſted every remedy which had been uſed. So great was the pain, &c. that it was imagined a ſtone was preſent, and the dreadful operation of lithotomy was thought of. When I firſt ſaw her, I ſaid it was nothing more than a diſeaſe of the mucous membrane of the bladder, conſtituting the dyſuria mucoſa of Cullen, and the catarrhus veſicæ of Lieutaud. Hoffman relates a ſimilar caſe (Conſultationes Med. Vol. II. p. 560) which he calls rarus veſicæ affectus. The ſoda pills gave complete relief. I attended the caſe with my ingenious friends, Meſſrs. Chapman, Surgeons, Ampthill. It is but juſtice to obſerve, that theſe gentlemen are ever conſpicuous, for an aſſiduous attention to their patients; and I have frequently derived conſiderable pleaſure from their acute obſervations.

APPENDIX, No. II.

LETTER *from the* AUTHOR *to Dr.* BEDDOES.

Dear Sir, *Bedford, Dec.* 11, 1797.

Your very polite letter of the 5th, I have had the pleaſure to receive. I have not been able to detect, in Mayow, the experiment on transfuſion you mention; and I am at a loſs to know, in what author you found it. The paſſage I allude to, is mentioned by you (in a note) in your Obſervations on Calculus, Sea Scurvy, &c. p. 104, and it is alſo quoted from you, by Dr. Thornton, in Med. Extracts, Vol. I. p. 126. I find one paſſage in Mayow, reſpecting transfuſion; but it is of another kind; it is as follows: ‘Enimvero ſi opii diſſolutio, aut aqua frigida, per venam jugularem injiciantur, cordis motus ſtatim crebrior erit, uti *ſæpe* expertus ſum.’ P. 303. The edition I have, is the collection of his tracts, publiſhed by himſelf at Oxford, 1674. If he was anxious enough to perform *repeated* experiments of this kind, it ſeems very probable, that his indefatigable zeal would have prompted him to transfuſe blood, from one animal into another; particularly as Lower had juſt diſcovered the method, and which was much noiſed abroad: and indeed in the year 1666, two years before Mayow gave to the world any of his works, the experiments of Lower, on transfuſion, were publiſhed in the Phil. Tranſ. (de Corde, P. 129.) So remarkable and curious a diſcovery, we ſhould imagine, would have arreſted the attention of Mayow; and if he does not mention it, it muſt appear rather extraordinary, particularly as it ſeems ſo much connected with his ſubject. I cannot find, in any author that I have conſulted, an obſervation, like the one in the quotation alluded to. I therefore diſtruſt my own accuracy, and the paſſage may ſtill be in Mayow, although it has hitherto eſcaped my examination. I have read, with ſome attention, almoſt the whole of Mayow's tracts, except the laſt, de Rachitide: it is not likely that the paſſage ſhould be there.

Dr. Scherer's Analyſis is in my poſſeſſion; it is a M. S. tranſlation from the German original, which I was favored with by Dr. Thornton. It is accurate; but does not give a ſufficiently full account of the beautiful ſyſtem publiſhed by Mayow. In the work of mine, about to make its appearance, I trace the hiſtory of air, as far back as the time of Hippocrates; and I remark a curious coincidence, between ſome of the doctrines of the Pythagorean

thagorean School (reſpecting air and water) and the modern diſcoveries on that ſubject. It is but juſtice, however, to obſerve, that my attention was directed to this circumſtance, by a letter which I received from Dr. Gregory, of Edinburgh.* Mayow I make my reſting place; and examine the progreſs of opinion, from his time to the preſent. The opinions of this great man, certainly, met with no encouragement in his own life time; for I do not find his name mentioned, by any one author, till after his death, when his doctrine became the prevailing faſhion of the times; and although greatly miſunderſtood by many, ſpread rapidly on the Continent, as I can prove, from a work which has come into my hands, (Schelhamerus de Nitro, and alſo from Morhof). The adoption of Mayow's theory of reſpiration, by the great Baglivi at Rome, contributed much to extend his reputation in Italy.

You, Sir, permit me to ſay it, are the man who firſt held up the works of Mayow to the philoſophic world. To you is ſcience indebted, for reſcuing from oblivion, the beautiful experiments of a great genius, who certainly ſhines, with all the ſplendor of a modern diſcoverer; and if the name of Mayow is mentioned, with reſpect, in literary circles, it is Dr. Beddoes who has reſtored him to diſtinction. You have promiſed to favor me with any thing that may occur, more ſatisfactory, on this ſubject, I ſhall thank you for the communication; and

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* Vide Introduction to Part Firſt.

I remain, &c.

To Dr. Beddoes, G. D. Yeats.
Clifton, near Briſtol.

Having reperuſed, with accuracy, Mayow's tracts, we diſcovered the paſſage, which had hitherto eſcaped examination; and which confirms the obſervations made in the above letter, *viz.* that it would have been a very extraordinary circumſtance, if a fact, ſo curious, and ſo much connected with Mayow's ideas of reſpiration, had not attracted his attention. We immediately communicated our ideas to Dr. Beddoes, from whom we received the following anſwer.

APPENDIX, No. III.

LETTER *from Dr.* BEDDOES *to the* AUTHOR.

Dear Sir, *Clifton, March* 13, 1798.

I cannot but be very thankful ſor your obliging letters, and rejoice, that your diligence has diſcovered the miſſing paſſage in Mayow. At the ſame time, with the beſt will poſſible, I cannot return your politeneſs in kind; for I have ſent my copy of Mayow to a perſon of more leiſure, to hunt for the account of the phænomenon in queſtion; and, therefore, cannot have the ſatisfaction of referring as you direct.

What you ſay about the air in ovo, excites my curioſity; but if I had Mayow, I could not have time to ſtudy him now; for beſides profeſſional occupations, I have embarraſſed myſelf with a courſe of chemical lectures.

If you can do it, without diſturbing the order of your matter, I wiſh you would introduce, from me, my own cenſure, on the manner in which I have abridged Mayow. It is too rhetorical a great deal; and beſides giving this proof of bad taſte, I have omitted conſtant reference to the pages of the original, which I hold to be requiſite, in a work, of which exactneſs is the principal merit.

I am,

Dear Sir,

With much regard, &c.

To Dr. Yeats,
Bedford.

THOMAS BEDDOES.

APPENDIX, No. IV.

LETTER *from the* AUTHOR *to Dr.* INGEN-HOUSZ, *relative to ſome curious Obſervations of* MAYOW, *mentioned in Sect. IX, Part Firſt, of this Work.*

Dear Sir, *Bedford, Jan.* 21, 1798.

When I had the pleaſure of meeting you in London, I mentioned to you, that Mayow, of the laſt century, entertained an opinion, ſimilar to your own, but which you firſt proved by experiment, *viz.* that the ſoil abſorbs oxygen from the atmoſphere. As I know you take delight in chemical inveſtigations, as ſoon as I arrived here, I ſat down to ſend you that paſſage from Mayow, which is very remarkable, for the expreſs manner in which he aſſerts this curious fact. This ingenious author, when ſpeaking on the neceſſity of a continual ſupply of fire-air to the reſpiration of animals, obſerves, ‘ Adeo enim ad vitam quamcumque ſal iſtoc aereum neceſſarium eſt, ut ne plantæ quidem in terrâ, ad quam aeri acceſſus præcluditur, vegetari poſſint, ſin terra iſta aeri expoſita, ſale hoc fœcundante denuo imprægnetur, ea demum plantis alendis iterum idonea evadet.

Plane ut vel ipſæ plantæ aliqualem reſpirationem aeriſque hauriendi neceſſitatem habere videantur.'

Does it not appear, from this paſſage, that the idea, that fallow was neceſſary for the abſorption of oxygen, preſented itſelf to Mayow? He has ſeveral other paſſages, to the ſame purpoſe. In my work, on the anticipation of modern diſcoveries, I ſhall take notice of this curious coincidence of thought, between you and Mayow. Do you not imagine, that oxygen, when abſorbed by the ſoil, enters into various combinations, for the food of plants? I know you have aſſerted this in your valuable publications; but you ſeem to attribute the moſt to carbonic acid.

You have mentioned ſome curious facts, in your ingenious letter to Sir J. Sinclair, with reſpect to the utility of alkaline ſalts in vegetation. Mayow has ſome remarkable ideas upon this ſubject. When you made the experiment with the acid, in Baron Dimſdale's garden, do you imagine any other effect to be produced by them, beſides diſengaging carbonic acid, by their union with calcareous earth?

Will not the oxygen, abſorbed in ſuch large quantities by the ſoil, aſſiſt in forming thoſe vaſt reſervoirs of water found in the bowels of the earth? Is it not partly intended for this purpoſe? I think it probable.

I remain, &c.

To Dr. Ingen-Housz. G. D. YEATS.

APPENDIX, No. V.

ANSWER *from Dr.* INGEN-HOUSZ, *containing an Account of his Diſcovery of the Food of Plants.*

Dear Sir, *February* 27, 1798.

I ſhewed your letter to Dr. G———, who was much pleaſed with the peruſal of it. I think the paſſage of Mayow very ſtriking, as are many others of that ſagacious man. You know, that Dr. Prieſtley and Mr. Scheele were of opinion, that plants thrive better in putrid, than in common air; and that vital air was hurtful to them. I diſcovered this to be a great miſtake; for I think I have proved, that the ſame air, which is ſalubrious to animals, is alſo ſalubrious to plants; and that plants will even live ſo much the longer in vital, than in common air, in proportion as the vital air is ſuperior, in purity, to the common air; and I point out a plain reaſon, why a plant, ſhut up in the dark, with a given quantity of common air, cannot be kept alive, but for a very limited time, though the carbonic acid be daily waſhed from the air; but that the ſame plant, ſhut up in the dark, with an equal quantity of vital air, (the carbonic acid being waſhed off) may be kept alive, as long as there is enough of that air remaining, to cover the plant. The death of the plant, in the common, and in the vital air, is produced by a very different cauſe. I do not know, whether you have ſeen the French edition of my work on vegetables, in two volumes, (there are three volumes of the German edition) where I explain this by facts, which you will find accurate.

As Mayow has anticipated ſo many things, diſcovered one hundred years after him, you may poſſibly alſo find, in the works of this great genius, what I have imagined to be my original diſcovery, about light and darkneſs. If you ſhould find that doctrine, I hope you will inform me of it, as I ſhould rejoice to have been, by accident, lucky enough to coincide with the opinions of ſo great a man. I do not want to repeat, what I here mention, as you muſt know it, *viz.* that plants correct injured air, and improve good air, by the aſſiſtance of the day or ſun's light. Dr. Prieſtley found, before me, that there was ſuch a power in plants; but confeſſed he was ignorant of the condition, neceſſary to make plants perform ſuch an office, as he and Mr. Scheele obſerved, that they very frequently injured air. Dr. Prieſtley, alſo diſcovered, that a ſlimy, filmy matter, ſpontaneouſly depoſited from well-water, had a power of producing pure air, in the ſun only; and Mr. Berthollet proved, that pure air is produced from oxygenated marine acid, by the ſun; which air was alſo found to be generated, by means of the ſun's light, from nitrous acid, by Mr. Scheele. But neither of theſe philoſophers have uttered a word, that theſe bodies were of a vegetable nature, or have drawn any concluſion from theſe remarkable facts; neither have they given it, as their opinion, that as theſe bodies give out vital air, by means of the ſun's light, that the effect of plants, in mending the air, was owing to the ſame cauſe; and, indeed, no one, in his ſound ſenſes, can doubt, but that they would have mentioned it; if they had known, or even ſuſpected it, before 1779, when I publiſhed my work on that ſubject.

The importance of the diſcovery, could ſcarce fail of exciting an envy, in a mind ſo laudably fond of philoſophical merit, as that of Dr. Prieſtley, and induce him to employ all poſſible ingenuity, to make his readers believe, that he found out the myſtery, by explaining the whole of it, without even mentioning my name. He was, however, not the only one, who laboured under the difficulty of finding out ſome turn of expreſſion, to draw the diſcovery to their ſide. Private letters were quoted by ſome, private converſations by others, and many other far-ſearched arguments. But to their mortification, not one of my competitors has ever ventured to ſay, openly, that they have publiſhed before me, that plants have ſuch power, by the aſſiſtance of the ſun's light. But you are, perhaps, ignorant of what ſtill puzzles ſome of my competitors, and puts them, even now, to a kind of torture; it is this, by finding, that a plant ſhut up with air had ameliorated it, it was impoſſible to conclude, from ſuch a fact alone, that it was the ſun's light, and not vegetation, as ſuch which had produced the effect, before examining another portion of the ſame air, after it had been expoſed to the influence of a ſimilar plant, in a ſhaded or dark place. Now the ſame teſt, by which it was diſcovered, that air expoſed to the ſun, became better, muſt have alſo pointed out, that the air, placed in the dark, was injured; and it was by this double examination I diſcovered both equally wonderful phænomena.

There is, indeed, more ſingularity in the effect of plants on air, in the dark, than in its effect on air in the day-light, as no one ever doubted, but that vegetation improves the wholeſomeneſs of countries. What other reaſon could be given, for the total ſilence of Dr. Prieſtley, on the effect of plants on air in the dark, but the unavowed averſion of quoting any ſuch examination made after my publication, as neither he, nor any one elſe, had even ſo much as ſuſpected, that ſuch a wonderful power exiſted in plants. Mr. Senebier, and ſeveral others, after having diſputed with me the fact (though it required but a few hours to verify it) during more than twelve years, and even declaring the doctrine to be a calumny againſt nature, have ſince publicly declared, that ſome inadvertency had made them fall into an error, and that they acknowledge Dr. Ingen-Houſz's diſcovery to be founded in the laws of nature. Dr. Prieſtley is the only one, who ſuppreſſes the whole nocturnal influence, by a profound, and as ſome think, an affected ſilence. The reaſon of this ſilence is obvious enough. I remember a learned profeſſor coming from Geneva (where Mr. Senebier had perſuaded him, that the doctrine of the nocturnal influence of plants on air was totally erroneous) addreſſed me in the following words, though, as you will eaſily believe, accompanied with a ſneer. ‘If the nocturnal influence of plants on air can be demonſtrated as true, it would be one of the moſt wonderful diſcoveries ever made; a diſcovery diſcloſing a phænomenon produced ſince the creation, by one half of the organiſed world, and which was not ſo much as ſuſpected by any mortal before you; it would make an epoch in natural philoſophy. But, my dear friend, continued he, I am ſorry to tell you, that you muſt not flatter yourſelf with ſuch an honour, for you will ſoon ſee, in Mr. Senebier's book (then in the preſs) how much you are miſtaken.'

taken.' As I am now in the country, without my books, I cannot point out to you, the places where I ſpeak of the air, as the nouriſhment of plants. My opinion has always been, and is ſtill, that air, abſorbed by plants, as well as water, ſalts, &c. undergo various chemical operations, and are variouſly modified, in their organs, as food is modified in the organs of animals. I repeat this doctrine, in my pamphlet on the food of plants, and the renovation of ſoils. Plants imbibe, by their leaves, atmoſpheric air, with all its ingredients; and this air is totally renewed, in leſs than half an hour, day and night. But I think, that the roots and the flowers imbibe a great deal of it, after it is changed into carbonic acid. I adduce facts to prove it; and as flowers prepare, inceſſantly, carbonic acid, to rear up the new offspring, I argue from it, that the principal part of the nouriſhment of plants, is air. I have thrown ſome more light on this my opinion, in a letter quoted in the pamphlet on the food of plants, addreſſed to Sir John Sinclair, Preſident of the Board of Agriculture;* but which makes no part of the pamphlet. There was, alſo, a further article on alkaline ſalts, as manure, in that letter, as alſo in a former. Theſe letters occaſioned a correſpondence between Sir John Sinclair and myſelf, who, through zeal for his ſociety, engaged me to put in writing, ſome further reflections on agriculture, which might occur to my mind, if they were but mere hints.

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* Lord Somerville is, we believe, now Preſident.

It would be too tedious, for you to read all my conceptions on the effect of acids, and other manures. My mind is ſometimes full of them; but I neglect to put theſe fugitive thoughts to writing, as not worth attention. You ſeem to think, by one of your queſtions, that, according to my opinion, nothing is to be expected further from acids, thrown upon the ſoil, but the production of carbonic acid, as you may, perhaps, conſider the union of the acid with the calcareous earth, as a permanent body; but this may be an erroneous opinion, as gyps expoſed to the chaotic maſs of vegetable ſoil and air, is eaſily decompoſed, and aſſiſts the putrid fermentation, neceſſary for promoting vegetation. I cannot anſwer your queries about oxygen, conceived by you as productive of vaſt quantities of water, as I never conſidered the queſtion. I think your experiment with the frog, very deſerving notice:* I hope you will continue theſe uſeful purſuits. You will excuſe me, in engaging ſo much of your time, in conſideration of my great eſteem for you; with which I profeſs to be,

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* A frog was made paralytic with mercury, in which the animal electricity did not produce its uſual effects.

Your's,

To Dr. Yeats,
Bedford.

J. INGEN-HOUSZ.

APPENDIX, No. VI.

LETTER *from Dr.* SHERER *relative to* MAYOW.

Sir, *London, March* 2, 1798.

Your kind and inſtructive letter, puts me under very great obligations, and much as I wiſhed to accept your liberal offer, I am ſorry to ſay, my time is too ſhort, to allow me the ſatisfaction of your perſonal acquaintance, and muſt, therefore, content myſelf, with having obtained it by correſpondence only. From all that I can collect, I conclude you to be a ſincere friend to chemiſtry, and this circumſtance alone, will probably be ſufficient to cement, when I am at a greater diſtance, your valuable acquaintance. I have dedicated all my life to chemiſtry, and my enthuſiaſm for it always increaſes, when I have the opportunity of forming acquaintances, ſuch as yours; and hope, we ſhall have, in future, means to communicate each others opinions upon chemical ſubjects; it being one of my firſt deſires, to continue my connections, by correſpondence with men of every country, where chemiſtry finds its admirers. I expect your publication, with the greateſt anxiety. *Mayow has not yet met the attention he merits,*

*merits. Weigel, Mertſger, and Haller, have but very imperfectly made uſe of his writings.** The paſſage of his, concerning transfuſion, I really do not recollect.† I flatter myſelf, your publication of Mayow will be complete. I ſhall not think of publiſhing my own, till I have read and ſtudied yours. In reſpect to Munday you are very right, and I was tempted by my nameſake to expect more from him.‡ My preſent departure for Paris, this day, occaſions ſo much confuſion in my mind, that I beg you to excuſe this unſatisfactory letter. With the utmoſt regard, I have the honour to add, the aſſurance of the ſincerity with which

I am, &c. &c.

D. ALEXANDER NICOLAUS SCHERER.

To Dr. Yeats,
Bedford.

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* The reader will ſee, by conſulting Mr. Cavallo's Treatiſe on Air, p. 508, that he alſo had a very imperfect idea of Mayow's diſcoveries.

† The paſſage concerning which the author wrote to Dr. Beddoes.

‡ Vide Introduction to Part Firſt.

FINIS.

DIRECTIONS to the BINDER.

THE HEAD of MAYOW to face the Title.

The other PLATES to be inſerted immediately before the APPENDIX.

CORRECTIONS.

P. 10, l. 10, *for* as, *read* ſo.—P. 12, l. 12, *for* form, *read* prove.—P. 14, l. 11, *for* liberty, *read* levity.—P. 29, l. 2, *for* attaching, *read* attacking.—P. 29, l. 12, *for* putrefaction, *read* putrefactive.—P. 47, l. 1, *for* become, *read* becomes.—P. 58, l. 11, *for* generaly, *read* generally.—P. 61, l. 13, *between* ſtring *and* to, *inſert* tied.—P. 81, l. 14, *for* hypothiſis, *read* hypotheſes.—P. 87, l. 21, *for* effuſion, *read* affuſion:—P. 89, note ‡, *for* digeſtion, *read* indigeſtion.—P. 90, l. 11, *for* ultimately, *read* intimately.—P. 91, l. 6, *for* fluid, *read* florid.—P. 93, l. 18, *after* allows, *expunge the comma*.—P. 93, l. 22, *for* in, *read* to.—P. 98, l. 3, *expunge* a.—P. 97, note *, *after* P. *add* 113.—P. 114, l. 7, *for* had, *read* has.—P. 117, l. 8, *for* event, *read* want.—P. 117, l. 16, *for* mutual, *read* maternal.—P. 110, l. 11, *for* air, *read* blood.—P. 122, l. 12, *for* is, *read* are.—P. 157, l. 18, *for* varies, *read* raiſes.—P. 159, l. 15, *after* ſhut, *add* up.—P. 168, l. 3, *for* nerves, *read* animal ſpirits.—P. 215, in the note, *for* of, *read* and.—P. 225, in the note, *for* tinged, *read* turgid.—P. 227, l. 2, *for* the, *read* that.—P. 241, l. 19, *for* reſt, *read* zeſt.—P. 244, l. 21, *before* the, *add* was.—P. 258, l. 3, *before* account, *add* an.—P. 282, l. 22, *for* or, *read* and.—P. 321, in the note, *for* Waller *read* Haller.—P. 325, l. 15, *for* ſhews, *read* ſhew.—P. 322, l. 7, *for* braided, *read* beaded.—P. 355, l. 7, *for* flowing, *read* glowing.—P. 359, l. 16, *for* to, *read* of.—P. 361, l. 17, *before* of, *add* is.—P. 384, l. 18, *for* Schereris, *read* Scherer's.